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THE MODERN OVARY: CONSTRUCTIONS, MEANINGS, USES

Chandak Sengoopta
The University of Manchester

In 1931, the doyen of British physiologists and pioneering endocrine researcher, Sir Edward Sharpey-Schafer (1850–1935; until 1918, Edward Schäfer), declared that physiology had recently undergone a fundamental reorientation. While the 'Old Physiology' had been based on the concept that bodily functions were regulated by the nervous system, the 'New Physiology', without underestimating the influence of the nerves, emphasized the chemical regulation of the organism.[1] Nowhere was the transition from nerves to hormones more remarkable than in theories of sexual physiology. Schäfer's own career attested to this momentous transformation: in 1895 he had remarked that the gonads exerted their influence on the body "without doubt ... through the nervous system" but in 1907 he considered it "highly probable that it is to internal secretions containing special hormones that the essential organs of reproduction — the testicles and the ovaries — owe the influence that they exert on the development of the secondary sexual characters ...".[2]

In this essay, I attempt to explore this transition in physiology by a series of focused studies on late nineteenth- and early twentieth-century studies of ovarian function. From the nineteenth century, medical scientists began to suspect that the ovaries — known, of course, since Antiquity — were more than simple reservoirs of eggs, but it took them years to agree on the nature and mechanics of these additional functions.[3] Experiments, therapeutic innovations, physiological debates, and rival interpretations of the ovaries' significance in the bodily economy kept the medical press humming until the early twentieth century, when it was finally agreed that the ovaries produced ova as well as internal secretions that regulated the female organism, its development and its life cycle. Until then, the enigmatic gland played dramatically different roles in diverse scenarios, reflecting the sheer number of intellectual and professional themes to which this one superficially familiar organ could be harnessed. The ovaries fascinated nineteenth- and early twentieth-century medical scientists for their ostensible relevance to intellectual, professional, or cultural issues having little necessarily in common with one another, and the ovaries' role in the chemical regulation of femininity came to be appreciated almost serendipitously in the context of investigations characterized by diversity, incoherence and contradictions.

My main focus here is on the *uses* of the ovary in discourse and practice across time and in relation to intellectual and broader, sociocultural contexts. What cognitive and clinical role(s) did the ovary play in medical scenarios during its 'neural' phase? What theoretical and practical goals did the neural model serve and in what kinds of professional contexts? Were these antagonistic to or supportive

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of the emerging chemical hypotheses?[4] No area of physiology was quite as messy, disorganized, controversial and undecided as the secretory functions of the sex glands between, roughly, the 1840s and the 1920s.[5] With hindsight, we can now see those elements in the chaos that were leading to today's endocrinology and it is certainly tempting to concentrate only on those. At the time, however, they had very different meanings, were intended for very different ends and interpreted in ways not necessarily endocrinological in our terms.[6] The evolution of today's endocrinology was anything but inevitable or even predictable: it depended on choices, compromises and chance. We are compelled to eliminate some of the most interesting and historically illuminating sections of the vast, complicated story of late nineteenth- and early twentieth-century glandular research if we try to tell it as chapters in the history of endocrinology.[7]

This, then, is the story of a gland and its transformations in medical discourse and *not* a straightforward contribution to the history of any particular medical field or discourse, whether endocrinology, physiology or gynaecology. I am concerned only with the ovary and try to explore as many different areas in which it was found to be significant: limitations of knowledge, time, patience and resources are the only reasons for omitting certain topics from consideration. A few clarifications on some of these omissions may be in order here. First, the purely reproductive functions of the ovaries and, for the most part, the contributions of the gland to such important processes as menstruation are ignored: they have their own complex histories that, given more time, knowledge and space than are at my disposal, can and should be integrated with the story I tell.[8] Second, although I do discuss some animal research, my chief topic is the human ovary and how it was understood by doctors and medical scientists. Those fascinatingly detailed studies of menstruation in the ferret or the vole that one encounters on the pages of Marshall's *Physiology of reproduction* I must leave aside.[9] Fourth, although I have not always confined myself to any specific national tradition and glanced more than occasionally at Britain and America, German research figures most prominently and consistently in the essay. This is because of its immense *international* importance at the time and, subsidiarily, because of the lack of detailed historical studies of these critical issues in recent English-language scholarship.[10]

"WOMAN IS WOMAN BECAUSE OF HER OVARIES"

"During the last twenty years perhaps no organ in the body has been so much written about as the ovary", remarked the gynaecological surgeon Lawson Tait in 1883. "Yet", he continued, "much remains to be discovered. To the naked eye nothing could look more uninteresting and unimportant than a human ovary; and yet upon it the whole affairs of the world depend. As far as the individual owner of the gland is concerned — certainly for her comfort, and, if we take with it its appendages, for her life as well, it is the most important organ in her body".[11] Nobody in the nineteenth century knew more about the ovaries — or was keener to find out more about them — than gynaecologists. Nelly Oudshoorn has rightly

observed that it was in "the gynaecological clinic" that the ovary was established as "the seat of femininity".[12] The gynaecologists who were most active in this area, however, were not the British peers of Lawson Tait but their Germanic counterparts. It is vital, moreover, to bear in mind that although gynaecologists enshrined the ovary as the seat of femininity, they did not *necessarily* conceive of the ovary's feminizing role as a chemical (i.e. hormonal) one. These and other crucial issues are best approached through a consideration of a gynaecological essay by the still young Rudolf Virchow.

In January 1848, the 26-year-old Virchow gave a lecture at a conference of the Berlin Society of Obstetrics.[13] The immediate occasion for the lecture was an epidemic of puerperal fever during the previous winter at the Charité hospital, where Virchow had been appointed prosector in 1847. Ostensibly, the lecture tried to outline a comprehensive "developmental history" of the puerperal state, which Virchow defined as lasting from menstruation to childbirth. Actually, it did much more and remains one of the most articulate expressions of the mid-nineteenth-century model of femininity, in which physiology and cultural imperatives were inseparably entwined.[14] Virchow's views were far from unique in content but unequalled in their cogency and impact, especially on medical thinkers.

Virchow spent considerable time discussing the cardinal function of the ovary, which, for him, was the production of the ova. He also believed, like many other medical scientists of the time, that menstruation was caused by ovulation.[15] What explained ovulation, therefore, would also explain menstruation. The ova, of course, ripened and were expelled only periodically — but what determined that cycle? Was the ovulatory stimulus communicated through the nerves or the blood? This was the crucial question, and in order to answer it one had to choose between those two basic orientations in physiology and pathology that are loosely describable as humoralism and solidism. The differences between these two perspectives, Virchow admitted, constituted one of the biggest divisions in the medicine of his time. The opposition of humoralism and solidism was crucial to the evolution of nineteenth-century pathology and the two orientations had ramifications that are too complex to be even touched upon here. For our purposes, we can define humoralism narrowly and simplistically as the view that within the body, actions at a distance were mediated by circulating fluids (primarily, of course, the blood) and solidism as the opposed conviction of distant action being mediated by solid structures, most notably the nerves.[16]

Virchow, of course, was renowned for his solidistic conviction that disease originated in solid, cellular structures but this conviction was not restricted to pathology. It shaped his views on the very nature of the body and its functions. Just as he opposed humoral theories of disease, he also heaped scorn on humoral concepts in physiology.[17] Unsurprisingly, therefore, he argued that if it was indeed the blood that caused ovulation, then one would have to decide whether the periodicity of ovulation — and menstruation — was caused by periodic changes in the composition of the blood itself or by periodic changes in the nutritive material

supplied to the ovaries by the blood. Nothing, he asserted, was known so far about the first alternative and there was no compelling reason to assume its occurrence. In any case, the blood itself could never cause the periodicity but merely be the conduit for some stimulus produced by an unknown tissue, which, he declared, was simply implausible. The most persuasive evidence against humoral theories, however, was a case of female Siamese twins who had lived for twenty-two years with what was essentially a single circulatory system but nevertheless menstruated at different times bearing no consistent relationship to each other. In the light of this case, it was far more likely that ovulation was caused by periodic changes intrinsic to the ovary, and what could be more natural, asked Virchow, than to assume that these periodic, rhythmic changes were mediated by the nervous system? The rhythmic processes of respiration and heartbeat and the contractions of the uterus during labour were all instances of neurologically regulated periodicity — it was simply logical to assume that ovulation and menstruation, too, were similarly regulated.[18]

The ovaries, Virchow continued, did not simply produce ova and cause menstruation: they generated femininity itself and all its attributes. To quote Virchow:

> The female is female because of her reproductive glands. All her characteristics of body and mind, of nutrition and nervous activity, the sweet delicacy and roundedness of limbs ... the development of the breasts and non-development of the vocal organ, the beauties of her hair and the soft down on her body, those depths of feeling, that unerring intutition, that gentleness, devotion and loyalty — in short, all that we respect and admire as truly feminine, are dependent on the ovaries. Take the ovaries away and we get the repulsive, coarsely formed, large-boned, moustached, deep-voiced, flat-breasted, resentful and egoistic virago (*Mannweib*).[19]

Virchow's formulation would be quoted and paraphrased down the years but as he had been quick to acknowledge, he was not alone in seeing the ovary as the fountainhead of femininity. Following the discovery of the ovum by Karl Ernst von Baer in 1827, the uterus had begun to lose its paramount position in medical theories of femininity.[20] Virchow had quoted the Paris physician Achille Chéreau (1817–85), who, in his 1844 treatise on the maladies of the ovary, had already dismissed Jan Baptista Van Helmont's influential dictum that woman was what she was because of the uterus ("*propter solum uterum mulier est quod est*").[21] Chéreau had proclaimed that it was the ovary that determined woman's nature and her body — *Propter ovarium solum mulier est quod est* — while the uterus was only an organ of secondary, purely reproductive importance. The ovaries were responsible for menstruation and it was ovarian dysfunction that caused such typically female disorders as hysteria and chlorosis.[22] Chéreau had desisted, however, from speculating on the mechanism of ovarian function. Even Virchow, who, as we have seen, provided an unequivocally neurological explanation of

ovarian function, had nothing to say about the specific nervous links between the ovaries and the rest of the organism, let alone the physiological processes by which nervous impulses could bring about and sustain "the sweet delicacy" of woman's form and spirit.

Virchow's invocation of the eternal feminine (more precisely, the glandular feminine!), however, was not simply a cloying and medically unimportant appendix to his neural theory of ovarian physiology. Femininity was created and guarded by the ovaries, which were governed by nerves, which explained as well as supported the nineteenth-century conviction that sensibility was fundamental to the very definition of femininity.[23] To use the words of Carroll Smith-Rosenberg and Charles Rosenberg, "few if any questioned the assumption that in males the intellectual propensities of the brain dominated, while the female's nervous system and emotions prevailed over her conscious and rational faculties".[24] Virchow's theory of ovarian function, then, did not simply explain reproductive periodicity by an historically specific model of physiological regulation, but placed the ovary and its putative physiology at the very centre of a historically specific model of ideal femininity.

THE GLAND UNDER THE KNIFE: THE OVARY AS NERVE-CENTRE

Virchow's views on the ovary and its role in generating and maintaining femininity were widely cited by later doctors — but not always in complete agreement. Some, in fact, came to regard the gland with suspicion and even frank hostility, identifying it as the culprit behind such 'female' disorders as hysteria, nymphomania, ovarian neuralgia, or the imaginatively named oophoromania in which the ovaries seemed to be outwardly 'normal'.[25] The removal of the ovaries became a popular treatment for these 'functional' conditions as well as in demonstrably organic ailments such as osteomalacia, intractable uterine bleeding due to tumours such as fibromyomas and, experimentally, even for breast cancer.[26]

Although the British surgeon Percival Pott had removed both ovaries of a patient suffering from a tumour involving both glands in 1775 and although the surgeon James Blundell had speculated about the usefulness of extirpating the ovaries in cases of dysmenorrhoea in 1825, the actual operation of removing apparently healthy ovaries (as opposed to grossly diseased ones) was independently introduced in 1872 by the American surgeon Robert Battey, the German gynaecologist Alfred Hegar (1830–1914), and the British gynaecologist Lawson Tait.[27] The stated rationale of the operation was that many 'female disorders' were accompanied by amenorrhoea and were actually manifestations of the organism's frustrated effort to menstruate; they could, therefore, be resolved if the source of the menstrual impulse, the ovary, were to be removed. I must emphasize here that there was no consensus among gynaecologists of the period about *how* the ovary caused menstruation.[28] The broader pathophysiological rationale for the operation, although seldom discussed in any great detail by its practitioners, was one of nervous regulation. In

the disorders concerned, the body's nerve force was supposed to be deranged due to amenorrhoea; the removal of the ovaries would restore the balance by precipitating menopause. The female organism, historians have repeatedly demonstrated, was seen by nineteenth-century doctors as being particularly finely balanced. Centred on reproductive functions, mature female existence — commencing at puberty and ending in menopause — was a succession of cyclically recurring biological processes and events: menstruation, pregnancy, parturition. This cyclicity was normal but it was also the fundamental cause of female instability. Virtually all the energy of the female organism — and every individual possessed only a fixed quantum of vital energy in nineteenth-century physiology — was needed to sustain this cyclical rhythm. The balance, even in normal conditions, was a fine one and the slightest alteration in the natural rhythms could jeopardize the entire organic economy of the female. Matters became particularly critical when menstrual flow ceased but the body continued trying to produce it.[29] The only cure was to remove the menstrual impetus by getting rid of the ovaries.

The proponents of the operation were unconcerned with elucidating ovarian physiology. Their motives were exclusively clinical: removing the ovaries, they were convinced, would cure certain conditions by creating an artificial menopause. Not every doctor believed in the operation's utility but nevertheless, by 1906, about 150,000 women are supposed to have had their 'normal' ovaries removed for a diverse range of illnesses.[30] One American surgeon who detested the operation remarked, "the woman who cannot show an abdominotomy line is looked upon as not in style, nor belonging to the correct set".[31] He was obviously exaggerating but there is some anecdotal evidence that occasionally, middle-class female patients, in the United States at least, demanded the operation from their gynaecologists even when the latter were relatively reluctant to recommend it.[32] Nor should one overlook the fact that even those who championed the operation took pains to warn their colleagues against its indiscriminate use. The German gynaecologist Alfred Hegar emphasized that it should be resorted to only after establishing unequivocally that the patient's physical symptoms were caused by an ovarian pathology. The simple presence of pathological ovaries was not enough for him: there had to be a clear causal link between the ovarian pathology and the patient's symptoms. "All operations which are undertaken without the presence of a disease or anomaly in the sexual system are, according to the present standpoint of our knowledge, unjustifiable", he declared. "The mere presence, however, of a pathological change in the genital system, as has commonly been held, is not sufficient, and a strict proof of the causative connection between that change and the nervous disorder has to be demanded."[33] The operation, he emphasized, was not indicated in neurotic symptoms unaccompanied by any "morbid changes in the sexual canal".[34]

It is important to bear in mind, however, that the majority of Hegar's indications were actually unrelated to nervous or mental symptoms and concerned with such conditions as uterine fibromyomas, profuse uterine bleeding or chronic, severe inflammation of the uterine tubes.[35] Hegar operated in cases of nervous disorders

only if they had not responded to a long-enough course of conservative treatment and only when they were accompanied by severe inflammatory conditions in the reproductive tract or serious menstrual irregularities, which, in his time, could legitimately be seen as the causal factors of the nervous condition.[36] Those who followed him were far more enthusiastic advocates of oophorectomy in nervous and mental disorders, although many of them stressed that the ovaries should only be removed in such cases if a local abnormality had been clearly identified in the reproductive system and linked to the nervous or mental symptoms. Oophorectomy in any case of hysteria was emphatically advised against by the leading exponents of the operation's merits.[37] As an admirer of Hegar's explained, as long as the symptoms of hysteria were linked to menstruation and an obvious abnormality in the reproductive system, then one could expect relief or even complete cure of the condition after oophorectomy.[38] There was wide consensus that while some cases of hysteria were due to lesions in the reproductive system, many were not and oophorectomy was not indicated in the latter group.[39] It must also be emphasised, however, that the category of nervous disorder or 'neurosis' was defined very widely in the nineteenth century, a point that Thomas Laqueur overlooks in his remark that Hegar believed that oophorectomy could cure hysteria and thus exorcise "the organic demons of unladylike behavior".[40] Neuralgias, cardialgia, cramps, vomiting, epilepsy or epileptiform convulsions, severe premenstrual discomfort of diverse kinds from coughs to back pain, hysterical paralyses, and not, as Laqueur seems to think, just the kind of flamboyant hysteria common in the clinic of Jean-Martin Charcot, were all classified as neuroses that could stem from problems in the reproductive tract.[41]

Pelvis, Brain and Nerves: The Ovaries and Female Neuroses

Hegar was certainly cautious and circumspect in prescribing the operation but he was firm in his allegiance to the neurological rationale that underpinned it. He regarded the ovary as a crucial node in the neural matrix of femininity. It was self-evident that when the ovary developed an irritative focus, the whole network, unstable even under normal conditions, would be seriously jeopardized. Conversely, an irritation developing in some other part of the neural network might implicate the ovary, causing the doctor to misidentify that gland as the origin of the irritation. "The nervous system", said Hegar,

> is so coherent a whole, that a transfer of the irritation, especially in the morbid state now under consideration, is very easily possible in all directions. An irritation applied to the ovaries may, therefore, start an attack which originally arose primarily in a reflex manner from some other part of the body, or which had its origin in an alteration of the centre.... Further sympathetic and reflex influences soon come into play, so that the first starting-point is, it must be confessed, often not easily found out.[42]

This implied, of course, that many nervous disorders, apparently unrelated to the sexual tract, might actually be linked to the latter, especially if the nervous symptoms

first appeared "in the nerves starting from the lumbar cord".[43]

This view was based on the nineteenth-century notion of spinal irritation, which held that impulses from an irritative focus in the reproductive system could radiate to the spinal cord via the nerves, from where they were disseminated all over the body, affecting diverse organs.[44] More specifically, Hegar and many gynaecologists of the time subscribed to Moritz Romberg's notion of reflex neuroses, identifying the female reproductive system (and especially the ovaries) as the ultimate source of apparently unrelated 'neurotic' symptoms, often categorized as hysterical, ranging from cardialgia to vomiting, from epilepsy-like convulsions to headaches.[45] 'Nervous' and mental disorders of women did not originate in the brain, let alone in the mind, but in the pelvis.[46] The Munich gynaecologist Joseph Amann (1832–1906) declared that anybody with practical knowledge of women's neuroses knew that the vast majority were brought about by female reproductive problems: any medical man who wished to avoid errors in diagnoses and prognoses of the neuroses and to treat them correctly needed the clearest conception of this causal nexus.[47] Amann did not, however, agree with Romberg's theory that all cases of hysteria were reflex neuroses originating in the genital tract: many cases of hysteria, he pointed out, were unaccompanied by any genital pathology and not all hysterical symptoms could be explained by a reflex mechanism.[48] The essential — though not the sufficient — cause of hysteria was an inherited predisposition: the disease was actually precipitated when certain releasing conditions (*veranlassenden Momenten*) acted upon a predisposed constitution.[49] Amongst those releasing factors of hysteria, diseases of the female reproductive tract were the most important as well as the most frequent.[50]

Psychiatrists did not dissent too radically from such conceptions. In his influential account of hysteria in Ziemssen's authoritative handbook, Friedrich Jolly (1844–1904), Professor of Psychiatry at Strassburg and later, successor to Carl Friedrich Otto Westphal's chair at Berlin, reminded his readers that hysteria was not an exclusively female disorder and hence could not be exclusively caused by female reproductive pathology. Nevertheless, Jolly emphasized that such abnormalities were indeed linked to hysterical conditions in various important ways. Even in healthy women, menstruation and pregnancy were often accompanied by nervous phenomena, demonstrating the specific links between the female reproductive organs and the nervous system. In some hysterics, symptoms were significantly worsened by menstruation or pregnancy, and in many cases, the correction of genital disorders relieved the hysterical symptoms. It was clear that in all such cases, the pressure, inflammation and irritation caused by local sexual problems were transmitted to the central nervous system by the centripetal nerves of the sexual organs.[51] Romberg's theory, although merely an updated version of the old Hippocratic uterine theory of hysteria, did contain a grain of truth.[52]

Despite such affirmations of the essential validity of the link between the nervous system and the female reproductive tract, oophorectomies in 'functional' cases went gradually out of vogue towards the end of the nineteenth century. The gynaecologist

August Rheinstaedter, who acknowledged that pelvic causes could indeed give rise to nervous and mental disorders in women, nevertheless declared that the rapid development of gynaecology had led to a "hysteromania" — every woman suffering from migraine or palpitations now believed that she had a uterine malady and could easily find doctors who would agree with her.[53] In their classic textbook of operative gynaecology, Döderlein and Krönig asserted that the removal of normal or slightly pathological ovaries in cases of hysteria was a practice of the past.[54] They added, however, that although it was no longer tenable to assume a direct aetiological relationship between genital pathology and hysteria, it would be wrong to dismiss any and every causal link between the two. They quoted the neurologist and psychiatrist Otto Binswanger — Professor of Psychiatry at the University of Jena and authority on functional nervous disorders — who, in a comprehensive 1904 monograph on hysteria, had emphasized that although "the modern exponents of the old uterine theory" were usually most imprecise in their conceptions of the genital causes of hysteria, and even though unthinking and prolonged treatment of gynaecological lesions could worsen hysteria, it was indisputable that the central nervous system could be affected adversely by inflammatory or degenerative conditions of the female pelvic organs. Brain, spinal cord and pelvis were linked by the cerebrospinal and sympathetic nerves.

Like Jolly twenty years previously, Binswanger doubted, however, whether such local genital abnormalities could precipitate full-blown hysteria in the absence of a pre-existing neuropathic disposition, which he conceptualized in the classic terms of degeneration theory as propunded by Benedict-Augustin Morel in mid-nineteenth-century France and expanded subsequently by Valentin Magnan.[55] Döderlein and Krönig added that in cases, for instance, of uterine bleeding, operative measures were indicated for hysterics and neurasthenics, who were far more likely to be affected mentally by genital problems than normal women. Similarly, chronic pelvic disorders of no great import in normal women could cause such severe, distressing symptoms in hysterics that surgery was often unavoidable. This surgery, however, was largely conservative, corrective surgery. Romberg's theory of hysteria, in short, was not quite dead yet but oophorectomy was no longer recommended for the condition.[56]

Why Keep the Ovaries?

What, however, of Virchow's point that the removal of the ovaries would turn a voluptuous and nurturant woman into a brazen virago? The oophorectomists either ignored that question or denied that the removal of ovaries in *adult* life could cause any generalized loss of feminine attributes, whether physical or mental. Hegar, for instance, regarded Virchow's dictum of woman being woman because of her ovaries as too dogmatic and unsupported by empirical observations. Castration of adult animals (such as cows and bitches) did not usually result in any typical changes in the sexual characters, except for a tendency toward excessive fat-deposition and, of course, the cessation of heat and menstruation.[57] The castration of women

rarely caused any significant changes in bodily form (with the possible exception of fat-deposition) or the secondary sexual characters. “We know little of the influence which castration exerts on the body as a whole, and on the nervous system”, declared Alfred Hegar. All he was certain of was that the removal of the ovaries stopped menstruation and ovulation and caused uterine atrophy — which, of course, were the very goals of the operation. The possibility of pregnancy was ruled out, although Hegar was unwilling to do so in absolute terms: all he asserted was that although exceedingly rare, “conception and even completion of pregnancy are possible. But the condition of the patient is such, that this eventuality is not even desirable”. Desire for coitus and the ability to have intercourse, however, were retained after the operation, which, Hegar emphasized, was one of the most important differences between male and female castration. In castrated males, “the ability to perform sexual intercourse is almost always lost.... The retention of the ability to perform sexual intercourse [in castrated women] also explains the fact that the mental depression, melancholia and even suicidal tendency, which occur in men after castration, are never observed in females”. Moreover, in women, “the mutilation is not visible and its entire significance is not recognized, because the illusion is not disturbed by constant inspection”.[58] One far more fervent proponent of the virtues of the operation even declared that for a man who did not want children, marriage to an oophorectomized woman would be the ideal Malthusian marriage, free from the impediments associated with artificial birth control methods![59]

As for the other attributes of femininity, Hegar expressed “very serious doubts whether the female type of configuration, the development of the breasts and of the external genitals, the tone of the voice, the peculiar mental tendency and mode of thought of the female are closely connected with the presence of the ovaries”.[60] Elsewhere, he declared that although babies born without ovaries or with damaged ones occasionally developed into women with impairments in the secondary sexual characters, this was far from the norm. Masculinization was an exceptional occurrence.[61] When such sexual malformations did occur, they were frequently accompanied by such unrelated features as idiocy, cretinism, and global deficits in the skeleton.[62] Conversely, striking degrees of masculinization could occur in the presence of apparently healthy ovaries. The only sexual attributes that were linked, albeit not absolutely and invariably, to the ovaries were menstruation, the development of the uterus, the uterine tubes and, to a smaller extent, the vagina — after bilateral oophorectomy, menstruation ceased and the uterus and its adnexae shrunk.[63] Hence, he suggested, one should not attribute all congenital sexual deficits to defective ovaries — it was more logical to assume that the development of the secondary sexual characters as well as the ovaries had probably been hampered in such cases by a broader underlying cause.[64] The sex glands, Hegar speculated, did not engender the secondary sexual characters but rather, both the gonads and the sexual characters were engendered by a ‘sex-determining force’ (*geschlechtsbedingende Moment*), the origin and nature of which was completely

unknown but which, probably, acted through the sex glands in most cases. The gonads, therefore, were important as mediators in the formative years but once the full adult form had been attained by the organism, their removal could not lead to any substantial, significant alterations of the sex characters except for the uterus, tubes and vagina.[65]

The ovary, then, was important to Hegar as a node in the female neural web and as a possible precipitant of nervous havoc but not as the exclusive seat of femininity. Hegar did not explain how the uterus and the uterine tubes atrophied after oophorectomy but others, who did not share his belief in the 'sex determining force', speculated that the removal of the ovaries, by terminating the regular nervous impulses from the glands to the nerve centres, interfered with the reflexes originating in the latter, which normally ensured the proper nutrition of the uterus by causing reflex hyperaemia. Alternatively, the atrophy could be due to the removal of possible trophic nervous impulses going from the ovaries directly to the uterus. The widely noted tendency to fat-deposition after oophorectomy was also explained, among other hypotheses, by nutritional imbalances within the body due to vasomotor disturbances consequent upon removal of the ovaries.[66] The nerves ruled the body (particularly the female body) and the ovaries were of central importance in that network. Details remained fuzzy but the idea itself was unquestioned. The notion of the ovaries producing chemical regulators of femininity was not even a theoretical alternative.

A Life-saving Operation?

Towards the end of the nineteenth century, the ovaries were dragged onto new, surprising arenas. Oophorectomy was introduced in the treatment of such mysterious and unrelated conditions as osteomalacia and breast cancer.[67] The theoretical foundations remained weak but the operation was found empirically beneficial, sometimes, apparently, more so than in the functional nervous disorders.

Osteomalacia

Osteomalacia, to put it simply, is a serious and progressive weakening of bones in adult life.[68] It was far commoner in the nineteenth century than it is today and encountered almost exclusively in women in their thirties.[69] The aetiology was unknown — although it seemed to German doctors to be endemic to some parts like the Rheinlands or Westphalia — and there was no effective treatment.[70] The condition was not only painful but caused severe skeletal deformities — although it progressed slowly and the course was punctuated by remissions, the patient was eventually bedridden and died of pneumonia or inanition. The condition was especially serious for women of childbearing age because pelvic deformities made childbirth difficult and dangerous and it was widely noted that pregnancy itself exacerbated osteomalacia.[71] Pregnant patients were often delivered by a modified caesarean section introduced by the Italian surgeon Edoardo Porro (1842–1902),

which combined the caesarean delivery with the removal of the uterus and ovaries so as to prevent future pregnancies. (One gynaecologist of the time wondered why future pregnancies could not be prevented by simple tubal ligations but his seems to have been a minority opinion.[72]) Porro had not introduced his operation for osteomalacia alone but for all cases where it was necessary to prevent future pregnancies. Cases of osteomalacia were often seen to improve after Porro's operation, which encouraged some surgeons to speculate that the removal of the ovaries directly benefited osteomalacia and should be performed even in non-pregnant patients.[73]

One of them was the leading gynaecologist Hermann Fehling (1847–1925), an admirer and associate of Alfred Hegar, co-founder of the *Zentralblatt für Gynäkologie*, and holder of professorial positions at Basel, Halle and Strassburg.[74] Having obtained impressive results in two of his own cases of Porro's operation, Fehling removed the ovaries of a thirty-six-year-old patient of osteomalacia who had been bedridden since the birth of her fourth child. By the time he reported on the case, she was fully mobile again and worked all day as a washerwoman.[75] Understandably, the report had significant impact and soon, other surgeons began to use the operation in osteomalacia. By 1889, Fehling himself had performed seven bilateral oophorectomies for osteomalacia, and between 1887 and 1906 more than two hundred patients of osteomalacia were treated similarly by others.[76] The number of reported cures was high: Jutta Blönnigen has estimated that of all the reported cases, as many as 63% were "cured" and 22% were "improved". Moreover, the improvements occurred rapidly after the operation and seemed to be sustained over long periods. Only in 6% of the cases was there no effect at all. On pathological examination, however, the majority of the removed ovaries did not present any conspicuous morbid features.[77]

Initially, the practitioners of the operation showed little interest in elucidating its mechanism of action or even in speculating on it. Fehling himself ignored the issue until the 1890s and then merely suggested that osteomalacia was caused by morbid hyperfunction of the ovary, which produced reflex venous hyperaemia by stimulating the sympathetic nerves. (Only in 1900 did Fehling suggest that internal secretory abnormalities of the ovaries might be responsible.[78]) The hyperaemia affected the skeleton by inducing the demineralization of bones and oophorectomy exerted its therapeutic effect by removing the pathological vasodilatation caused by hyperactive ovaries. The notion of a hyperactive ovary was supported by Fehling's observation that patients of osteomalacia were remarkably fertile but except for that correlation, oophorectomy in osteomalacia was justified solely by its empirical success.[79] Fehling's neural explanation of osteomalacia as a "trophoneurosis" of the bones was accepted by many practitioners and even the few critics did not proffer an endocrine hypothesis before Fehling himself did.[80] Franz von Winckel, for instance, argued that although osteomalacia was often worsened during menstruation, it also flared up in its absence, as during pregnancy and lactation. While it was true that the ovaries were not completely inactive during pregnancy, it was logical to assume

that they were not as active as during menstruation — if, then, osteomalacia, was caused by the ovaries, then one would expect it to improve during pregnancy and lactation, which was far from the case. Oophorectomy, von Winckel argued, probably worked by reducing the sensitivity of the muscles and the periosteum, rather than by removing the fundamental cause of the disease.[81] Others suggested that osteomalacia was caused by bacteria — there was even a serious suggestion that the curative effects of oophorectomy were due simply to the chloroform used for anaesthesia, which killed the nitrifying bacteria responsible for the condition.[82]

When endocrinological notions finally made a real mark on the subject of osteomalacia, it was from the opposite direction: ovarian extracts were used to *treat* the condition! This, of course, was diametrically opposed to Fehling's rationale and was justified with the argument that it was *abnormal* ovarian secretions that caused the disease and the only logical way to treat it was with *normal* secretions.[83] The other endocrine glands also came to be implicated and finally, the disease was attributed to pluriglandular deficiencies.[84] Nevertheless, reports of bilateral oophorectomy in osteomalacia continued to be published almost until 1920 and the procedure continued to be recommended in textbooks.[85] As an early exponent put it pithily, however, oophorectomy in osteomalacia — in spite of all hypotheses — remained a purely empiric treatment and the mystery of the disease, in some ways, had actually been deepened by the success of Fehling's procedure.[86]

Breast Cancer

In 1896, George Thomas Beatson (1848–1933), Visiting Surgeon to the Glasgow Cancer Hospital and a former house surgeon to Lord Lister at the Edinburgh Royal Infirmary, published an account of his treatment of three patients suffering from breast cancer.[87] Beatson argued that breast cancer was analogous to lactation:

> we have, under both these conditions, the same proliferation of generations of epithelial cells which block the ducts and fill the acini of the gland; but in the case of lactation they rapidly vacuolate, undergo fatty degeneration, and form milk, while in the carcinoma they stop short of that process, and, to make room for themselves, they penetrate the walls of the ducts and the acini and invade the surrounding tissues. In short, lactation is at one point perilously near becoming a cancerous process if it is at all arrested.[88]

He was also greatly struck by reports that "it is the custom in certain countries to remove the ovaries of the cow after calving if it is wished to keep up the supply of milk, and that if this is done the cow will go on giving milk indefinitely". British farmers did not spay their cows but negated the action of the ovary by encouraging regular pregnancies, during which, Beatson surmised as had von Winckel in the context of osteomalacia, the ovaries were "functionless — that is to say, we have not the indications of its activity in the shape of the menses". Menstruation, then, opposed lactation but the secretion of milk, Beatson pointed out, was not controlled

by any "special nerve-supply of its own" and could not be affected by interference with the sympathetic or the spinal nerves. In the 1870s, Beatson decided to test his hypothesis on rabbits. He did not describe his experiments in any detail but summarized his results thus:

> As long as the young ones were at the breast [of the oophorectomized mother] the milk-supply continued, and when eventually they were taken away the milk-supply ceased: but the creatures increased very much in size, and post-mortem examination revealed that this was due to large deposits of fat around the various organs, and above all, in the lumbar region, where there were masses of pure adipose tissue, showing that the secretion of milk was still going on, but, not being discharged by the usual channels, was deposited in the various tissues of the body as fat.[89]

Lactation, therefore, was probably controlled directly by the ovary — and, as he had suggested, lactating breast tissue was analogous to malignant breast tissue. The only significant difference between the two was that the former continued to a stage "where the cells became fatty and and passed out of the system not only in an innocuous but nourishing fluid — milk". If, now, the suspension of ovarian influence (as during pregnancy) induced lactation, then would surgical removal of the ovaries stop the neoplastic process in breast cancer by compelling it to proceed to the fatty degeneration seen in lactation?[90]

Beatson now shelved this hypothesis for almost two decades because of his aversion to experimenting on human beings and because of contemporary hopes that the advent of bacteriology might soon resolve the cancer problem by showing it to be due to an infection of some kind. Finally, however, he relented upon receiving a patient who had already had a mastectomy but whose tumour had recurred, rapidly reaching inoperable status. At first, he treated the patient with thyroid extract in the hope that it might hasten the mucoid degeneration of the cells and thus affect the tumour. That did not occur and Beatson finally returned, with his patient's consent, to his old idea of oophorectomy. Thyroid tablets were continued after the operation, which were now administered in the belief that the removal of the ovaries would cause fatty degeneration of the tumour cells and since the tumour was very large, the lymphatic system might be overburdened unless stimulated by a "powerful lymphatic stimulant". The patient improved quite significantly and four months after the operation, Beatson noted, "the cancerous tissue has been reduced to a very thin layer" and eight months later, "all vestiges of her previous cancerous disease had disappeared".[91]

In his paper, Beatson presented the cases of two other patients, both with tumours impossible to resect. After oophorectomy and thyroid administration, microscopic examination of the tumour tissue showed extensive fatty degeneration of the epithelial cells.[92] There was no dramatic cure, however, and Beatson merely suggested that "the disease is in a more quiescent stage". The third patient had already gone through menopause and hence Beatson was initially not very hopeful

about the impact of oophorectomy. Instead, he first prescribed thyroid extract alone but to little effect, and at the time of his lecture he was debating whether, given his suspicion that "ovarian irritation may be the exciting cause of cancer", it would not be appropriate to remove the ovaries and uterine appendages, despite the patient's being post-menopausal. Five years later, he reasserted his faith in the value of oophorectomy and thyroid extract for inoperable cases: the results were sometimes so dramatic that once, the patient's physician had observed, "This seems like a romance"! Other surgeons had also begun to report similar successes with the procedure. Sometimes, Beatson emphasized, supposedly "spontaneous" cures of cancer were actually attributable to menopause.[93]

What did he conclude from all this? First, that the ovaries were linked to breast cancer and perhaps to all malignancies in the female. More broadly, he speculated whether it was correct to attribute "the entire regulation of the metabolic changes in the tissues of the body" to the nervous system. "I am satisfied", he declared, "that in the ovary of the female and the testicle of the male we have organs that send out influences more subtle it may be and more mysterious than those emanating from the nervous system, but possibly much more potent than the latter for good or ill as regards the nutrition of the body".[94] These mysterious influences of the gonads, he hypothesized, normally prevented the somatic cells from turning into undifferentiated, 'primitive' germinal cells, capable of unchecked proliferation. (Beatson observed that he "had never been sure" about August Weismann's absolute separation of the soma from the germ-plasm.) In conditions of "altered secretion" or "any morbid condition" of the ovaries, the inhibitory influence on the somatic cells was removed, enabling the latter to turn, in effect, into primitive germ cells. Cancer cells, Beatson suggested, "will eventually be shown to be special germinal cells corresponding to the ovum-cells elaborated by the ovary". But what exactly was the nature of the ovarian influence and what specifically changed in cancer? This is where Beatson was frustratingly enigmatic. "It may be an altered secretion", he surmised, "or it may be the migration of cells — it might even be a parasite in the ovarian cells, for it should be borne in mind in regard to the secretions of the reproductive glands 'that unlike other secretions, their essential constituents are living cells' (Stewart)".[95]

Had Beatson intuited the endocrine functions of the ovary before the supposed 'discovery' at the turn of the century? Although his hypothesis referred to ovarian "secretions" and argued against the exclusiveness of nervous regulation of the body and its metabolism, his approach as a whole was clearly based on the reproductive functions of the ovaries and testes. The secretions he referred to in his somewhat ambiguous sentences were the germinal secretions, as Hans Simmer has shown by tracing the original physiological text by George Stewart, from which Beatson had quoted.[96] The era of internal secretions had dawned by 1896 but for Beatson at least, the ovary was still the ovary of yore — a reproductive gland rather than a reproductive *and* an internal secretory gland, even if it had possible important influences beyond the reproductive sphere. It was only in 1905 that he referred to

internal secretions and then only casually, when he reported that in the light of recent reports about the corpus luteum producing "an internal secretion which causes the fixation of the embryo to the uterine wall and thus controls cell proliferation", he had unsuccessfully experimented with luteal extract in breast cancer.[97]

The next episode in the ovary-cancer saga was also British but here, at last, we find a conception of ovarian physiology that even the least whiggish historian would have to acknowledge as modern. The protagonist here is the surgeon James Stanley Newton Boyd (1856–1916), senior surgeon to the Charing Cross Hospital, who, ironically, seems to have been completely overlooked by the recent medical champions of Beatson's supposedly "endocrine" treatment of breast cancer! Building on Beatson's reports and on other instances of a "spontaneous" remission of breast cancer after menopause, Boyd presented five cases of his own in 1897.[98] Not all his cases were successful but nevertheless, Boyd was confident in claiming a causal link between oophorectomy and the diminution of tumour size; but more importantly for us, he hypothesized that "the internal secretion of the ovaries in some cases favours the growth of the cancer, acting either upon the epithelial cells or upon the surrounding tissues".[99] (He wondered whether castration would have similar effects on cancers in men, but although the gonads developed from the same embryonic rudiments in the two sexes, "nothing", asserted Boyd, "can be clearer than that the ovary and testis differ in most respects".[100])

In 1900, Boyd presented an analysis of fifty-four cases of oophorectomy in breast cancer, compiled from his own work as well as from that of others.[101] Nineteen of these cases (34%) improved after the operation, thirty-four were unaffected or benefited only marginally, and one died of a cause unrelated to the cancer. Depressingly, however, it seemed that even in the cases responding to the operation, the cancer tended to recur within a year in the majority. Nevertheless, "a year or more of useful life seems often to have been gained". It was difficult to explain the wholly unresponsive cases and Boyd speculated that "certain ovaries, probably by pathological variation in their internal secretion, favour the growth of cancer ... the removal of such ovaries alone will be of benefit". He had not, however, discovered any signs that might indicate this crucial difference, whether clinically or microscopically.[102]

Boyd was undoubtedly thinking in terms that we can justifiably call endocrinologic. His concerns did not, however, extend to the physiology of femininity. He mentioned very briefly that after oophorectomy, amenorrhoea and atrophy of the uterus and the breasts were constant. Sexual feeling remained unaffected but occasionally, premature ageing, growth of facial hair or flushes were reported.[103] The great early twentieth-century concern with the relation of internal gonadal secretions to the secondary sexual characters was entirely absent in Boyd's work. And understandably so. His interest in internal secretions of the ovaries was purely clinical, even surgical and belongs to the history of surgery and cancer therapeutics, rather than to the history of endocrinology.

THE AGE OF THE INTERNAL SECRETIONS

It is usual to date the beginning of endocrine era from 1889, when the physiologist Charles-Edouard Brown-Séquard declared that he had "rejuvenated" himself by injections of testicular extracts.[104] This led to a craze for treating hitherto incurable disorders with extracts not only from the sex glands but from every imaginable organ. Soon, however, the many flamboyant and unsubstantiated claims of Brown-Séquard, his followers, and other exponents of magical cures by 'organotherapy' stigmatized the entire field in the eyes of many medical scientists. Referring to that phenomenon, the endocrinologist Herbert Evans declared in 1933 that "endocrinology suffered obstetric deformation in its very birth".[105] However, as historians, in particular Merriley Borell, have pointed out, Evans and the many who shared his views were less than generous in assessing the contributions of the early 'organotherapists' toward the rise of endocrinology as a science and a distinct medical speciality.[106] Professional endocrinologists have tended to portray early clinical efforts at organotherapy as little better than quackery. And yet, the organotherapeutic perspective was of pivotal importance in the genesis of experimental research that has been universally recognized to be of foundational importance to endocrinology.

Vienna, Fin de Siècle: The Case of the Missing Ovaries

In 1922, William Seaman Bainbridge, a New York gynaecologist, declared in a lecture: "There was a period when excision of the ovary was the rule rather than the exception and a cystic ovary was a surgically doomed ovary, but the pendulum has swung in the opposite direction.... Happily, the ovary instead of having to prove its right to remain seems now in a position to demand of the surgeon the reasons for its removal."[107] Bainbridge is not an important figure in this story and I quote him only as a representative of the new gynaecology that had suddenly begun to treat the ovaries with great respect.[108]

How did this consensus arise and what was its intellectual basis? To put it simply, it was a conjoint product of the laboratory and the clinic — with the clinic being the more important partner — and in conceptual terms, led back to the world of nineteenth-century organotherapy. Crude extracts from the sex glands had come into clinical use promptly after Brown-Séquard's announcement. All kinds of doctors prescribed them for all kinds of ailments, real or imagined.[109] Brown-Séquard himself had reported the beneficial effects of ovarian extracts in prematurely senile women and in hysterics. Those early recommendations led to their use on patients whom the endocrinologist George Corner later dismissed as "hysterical girls and cachectic women".[110] As Hans Simmer has shown, however, interest in ovarian functions and the organotherapeutic supplementation or substitution thereof was not found solely among general practitioners trying out ovarian extracts on patients with ill-defined mental or 'functional' symptoms.[111] Gynaecologists were preeminent in this research and many of their successes are hailed as epochal by all histories of

endocrinology. This is best illustrated by an analysis of one of the most significant early contributions toward the understanding of ovarian functions: experiments on ovarian transplantation conducted in Vienna during the last decade of the nineteenth century by the gynaecologists Emil Knauer and Josef Halban.

The received version of their work, expressed most influentially in George Corner's previously cited lecture, "The early history of the oestrogenic hormones", draws a sharp distinction between earlier efforts at organotherapy with ovarian substance and the "more solid kind of investigation" attempted by Knauer and Halban.[112] Corner's lecture continues, rightly, to be a popular source for the history of research on the ovaries, but this contention of his does not stand up to scrutiny. In the late nineteenth century, as we have seen, numerous women had their ovaries removed for a variety of disorders. Any gynaecologist working at a large teaching hospital necessarily encountered oophorectomized patients returning to the doctor with symptoms of premature menopause. Central European practitioners approached the condition with great seriousness and Knauer's experiments, eventually of such pathbreaking significance for endocrinology, evolved out of that tradition.

Knauer's story begins with an observation of his superior, Rudolf Chrobak (1843–1910), who was unique in having received his training in gynaecology in a Department of Medicine, and fortunate in having studied experimental physiology with the charismatic Ernst von Brücke (1819–92), whose laboratory, according to Erna Lesky, was an "almost inexhaustible center of stimulation of Vienna medicine in the second half of the century". Chrobak's medical and physiological sophistication alerted him to issues that were rarely addressed by average gynaecologists of the time, whom Lesky dismisses as mere "uterine engineers".[113] Around 1895, Chrobak declared that his satisfaction in conducting meticulous oophorectomies (often accompanied by hysterectomies) had been soured (*vergällt*) by the large number of patients who complained that they felt much worse after the operation than before it. The symptoms, Chrobak was convinced, were largely due to the loss of the ovaries and although they broadly resembled those of natural menopause (dizziness, headaches, hot flashes, sweats), their intensity was incomparably higher. He had been so impressed by the frequency and the severity of the symptoms that he had already started sparing the ovaries in some operations where they were routinely removed with the uterus. While he could offer no statistical evidence, he was confident that such conservative operations produced fewer menopausal symptoms.[114] Chrobak then approached the issue from another angle. He was well aware of recent successes in treating symptoms of thyroid deficiency with extracts of the thyroid gland. (From today's perspective, thyroid and adrenal extracts were the only biologically effective agents in the nineteenth-century organotherapeutic armamentarium.[115]) Chrobak reasoned, therefore, that the administration of ovarian substance to oophorectomized patients could be useful.[116] He does not seem to have employed it too extensively, however, and reported only moderate success.[117]

Neither Chrobak's analogy between the thyroid and the ovaries nor his therapeutic procedure was a radical innovation, although he himself does not seem to have been familiar with the contemporary popularity of identical concepts. The year 1895, in fact, seems to have been particularly crowded with independent and virtually simultaneous 'discoveries' of the ovary-thyroid analogy.[118] Hans Simmer has identified two gynaecologists, apart from Chrobak, who argued in 1895 that ovarian deficiency was likely to be comparable to thyroid deficiency and should, therefore, respond to treatment with ovarian substance. Their studies were published almost simultaneously with Chrobak's, reporting broadly similar results.[119] Although the ovarian preparations used in these studies were unstandardized and differently prepared, the results of the treatment were reported to be good as far as the associated symptoms of menopause were concerned. Sexually specific effects, such as the resumption of regular menstrual cycles, were, however, not obtained.[120] (I should point out here that modern endocrinologists find the beneficial results impossible to explain except as placebo effects since the ovarian hormones are insoluble in water and present in minute quantities in ovarian tissue. My point, however, is merely that organotherapy with ovarian extracts was *believed* to work, although not always very reliably, by gynaecologists and their patients.) It was from within this organotherapeutic context that Rudolf Chrobak took his next step, which endocrinologists and historians of endocrinology consider to have led to the birth of the modern concept of the ovary.[121] Unsatisfied with the reliability of extracts, the physiologically sophisticated Chrobak wondered whether it would not be better if, instead of administering ovarian substance to the patients, one could replace their missing ovaries by transplantation. Transplantation was not, therefore, conceived of as a physiological experiment but as a more effective form of organotherapy.

Once again, Chrobak compared the ovary to the thyroid and argued that since thyroid deficiency was by now known to be relieved by thyroid grafts, it was possible that the signs of ovarian deficiency might be removed by the surgical transplantation of ovarian tissue.[122] But was ovarian transplantation even possible? In 1895, Chrobak asked his assistant Emil Knauer (1867–1935) to conduct experiments on rabbits to test its practicability and utility.[123] These experiments were designed only to test a therapeutic notion: the functions of the ovaries were not directly under investigation and Chrobak did not speculate on the possible mechanism of the ovary's action upon the female organism. Nor did Knauer until later.[124] This reserve is all the more remarkable because one could not really believe that ovarian extracts or transplantation could work unless one thought that the actions of the ovary were mediated by chemicals.

Knauer began by removing the ovaries from four rabbits and then transplanting them elsewhere in the abdominal cavity of the *same* animal. These autografts 'took' well and in one case, even six months after the transplantation, the internal reproductive organs did not show signs of atrophy when examined at autopsy. Microscopic examination showed that the grafted ovaries were histologically

normal and contained the usual numbers of follicles, some with clearly visible ova in different stages of development. The experiments, therefore, suggested that (a) the ovary could be successfully transplanted in locations different from its anatomical site, and (b) the transplanted ovary remained functional.[125] But exactly how functional and for how long? Was pregnancy possible in an animal with transplanted ovaries? More importantly, given the therapeutic aims of the experiment, would it be possible to replace the ovaries of one animal with those of another?[126]

Knauer soon succeeded in inducing pregnancy in a rabbit, the ovaries of which had been removed and reimplanted within its abdominal cavity sixteen months before mating. The pregnancy continued to term and ended in a natural birth. A second effort on the same animal failed but probably due to sterility in the male partner.[127] Three years after the transplantation, the rabbit was seriously ill and feeble and Knauer decided to kill it. On autopsy, the findings exceeded Knauer's expectations: the reproductive organs were normal and so was the microscopic appearance of the transplanted ovaries. Transplanted ovaries, therefore, could remain fully functional for a significantly long period — which, for a rabbit, was almost as long as the normal duration.[128] Knauer fared less impressively, however, in his attempts to transplant ovaries from one animal into another. Of his initial series, all the grafts except two underwent immediate necrosis, disappearing completely at the end of a year. The uterus, expectedly enough, had atrophied and so had the external genitalia. Of the two more successful cases, one died after three weeks and at the time of death, one of its ovarian grafts was still functional. The second rabbit lived with the grafts for a year and a half but its reproductive organs underwent atrophy and on microscopic examination, the ovarian tissue did not contain any follicles.[129]

Josef Halban: The Ovaries, Development and Menstruation

In the discussion following Knauer's lecture on these experiments, another young Viennese gynaecologist, Josef Halban, recounted his own, independent experiments on the subject of ovarian transplantation.[130] Although Halban is universally acknowledged as one of the pioneers in the history of ovarian endocrinology, the precise contexts of his research are often obscured. The fundamental and somewhat paradoxical feature of Halban's approach was that although he went on to become one of Vienna's best-known clinical gynaecologists, his experimental research on the ovaries, conducted in the early phases of his career and always mentioned in conjunction with Knauer's clinically-oriented research, had no discernible clinical motivations at all. It was Halban's aim to study the influence of the ovaries on the development of the female reproductive system and to speculate on the nature, extent and mechanism of that influence.

Beginning in 1897, Halban removed the ovaries, the uterine tubes, and a piece of the uterus of newborn guinea-pigs and then reimplanted them under the skin in the experimental group. The control animals did not receive any transplants and their

uteri remained undeveloped but did not atrophy. The experimental animals, on the other hand, developed normally and even after more than a year, no problems were encountered in the development of the reproductive organs and the breasts — even the uterine tubes transplanted under the skin with the ovaries remained functional. It did not matter, therefore, where the ovary was — its presence anywhere in the body ensured the proper development of the reproductive system. Furthermore, Halban emphasized, the infantile uterus did not atrophy in the absence of the ovaries but simply failed to grow. The ovaries, therefore, did not just sustain the functions of the adult uterus but directly governed the development of the reproductive organs into their adult form. Previous ideas of a neurological link between the uterus and the ovaries were untenable: no such link could conceivably be restablished after the transplantation of the ovaries under the skin. "We must therefore", declared Halban, "explain this phenomenon by the internal secretions, the nature and attributes of which, admittedly, are still wholly mysterious. These experiments compel us to assume that a substance is secreted from the ovaries into the circulating blood, which is capable of exerting a specific influence upon the rest of the genital system".[131] Halban's experiments were illuminating but his greater contribution was to provide this new interpretive framework for Knauer's transplantation experiments — and, indirectly, for organotherapy with ovarian extracts. A clinically oriented organotherapy, then, was not succeeded by a laboratory-bound endocrinology. Rather, the latter grew out of the former and at least during this period, clinical aims still determined the direction of laboratory research but the most persuasive explanation of that laboratory research came from a clinician who, at that point in time, approached the problem solely from a developmental biological standpoint.

Knauer's demonstration of the feasibility and efficacy of ovarian transplantation aroused the interest of clinicians: within a year of its publication, his long 1900 paper was reviewed in detail in far-off Pennsylvania by a gynaecologist. The reviewer was optimistic about the future applicability of the operation, while emphasizing that it was far easier to spare the ovaries in an operation than to transplant ovaries later.[132] A large number of researchers, in continental Europe as well as the United States, conducted many experimental ovarian transplantations, in animals as well as humans.[133] The transplantation of ovaries from its original site to another in the same individual (autotransplantation) was usually successful in maintaining menstruation and preventing the symptoms of premature menopause. This could obviously be very useful in human cases where a serious pelvic infection (commonly gonorrheal in the early twentieth century) made it unsafe to leave the ovaries *in situ*.[134] Occasionally, successes were reported in the transplantation of ovaries between individuals but generally, this proved far more difficult than autotransplantation and according to some, practically impossible in humans. Although a few good results were sometimes reported, the latter remained a deeply unsatisfactory procedure.[135] The new emphasis on conservative surgery, in any case, dramatically reduced the need for transplantations. Chrobak's original attempt to

find a surgical solution to the problem of premature menopause, therefore, led much more to the enrichment of physiology rather than of clinical gynaecology.

It was undoubtedly with Halban's work that the ovary began to be transformed convincingly from a mysterious, neurologically controlled (and controlling) gland into a source of internal secretion or possibly secretions, the chemical composition(s) of which were mysterious but whose functions were definable with some clarity. And yet, this same Josef Halban spent the rest of his illustrious career in arguing that the sex glands merely exerted a protective influence on the sexual attributes: the sexual characters were *generated* independently *ab ovo*. (This last assertion was merely an assertion supported by circumstantial evidence: Halban offered no genetic argument at all.) By then, many other scientists, including the notorious Eugen Steinach of Halban's own Vienna, were arguing for the preeminence of the sex glands in determining and maintaining the characteristics of sex and it was Halban, but for whose work the history of endocrinology might have been very different indeed, who was their greatest opponent and never seems to have had a second thought about his mature position on the issue.

Did Halban's early research, however, convert other scientists to the endocrine hypothesis of sex in spite of his own reluctance to go all the way? The state of historical research on this period is so unsatisfactory that any answer to this question must, necessarily, be somewhat tentative. The basic problem seems to have been that transplantation experiments could not *absolutely* rule out a certain 'preformation' of the sexual characters, as had, for instance, been argued by Hegar and later, by Halban himself: the uterus may have failed to develop in those of Halban's experiments where the ovarian graft did not survive but it did not atrophy. Secondly, it was improbable but not entirely impossible that the results of glandular transplantation might be due to the reestablishment of nervous connections between the graft and the rest of the organism. In 1905, Moritz Nussbaum offered a rather strained compromise by suggesting that although sexual development could not occur in the absence of the internal secretions of the gonads, those secretions did not act directly on the end-organs but, rather, through the nerves.[136] As fervent a champion of the internal secretory paradigm as Alexander Lipschütz was compelled to admit, as late as in 1924, that "transplantation experiments cannot absolutely decide the question whether the sexual glands act by nervous reflexes or by internal secretions discharged into the circulating blood".[137]

The question here, it should be reiterated, was one of absolute certainty: few denied that the probability that the sex glands influenced the sex characters through secretions was high. Furthermore, even if one believed in the sexual omnipotence of the internal secretions, one still had to admit that their exact histological sources, their nature and their actions all remained unclear. "Actually, at the beginning of 1923", recalled George Corner, "before Allen and Doisy's first paper appeared, the literature of ovarian endocrinology was in a very confused state" and he, as he himself realized, was one of the less severe critics.[138]

ONE GLAND INDIVISIBLE OR MANY GLANDS IN ONE?

One basic question that would dog ovarian research well into the twentieth century was that of the histological source of its putative secretion — or perhaps secretion*s*. The ovarian research we have discussed so far had little, if anything, to say on exactly which part of the ovary the internal secretions came from. This was not simply an academic question, however, since the ovary was not a simple, homogeneous piece of glandular tissue: it was structurally complex and to make matters even more complicated, its structural features changed significantly at different points of the female cycle. Histologically, the ovary at the beginning of the menstrual cycle was different from the ovary after ovulation; the ovaries of pregnant women differed histologically from those of a pubescent maiden. In both these categories, the major difference was that after the ovum was expelled from the ovarian follicle, the ruptured follicle was converted into a cellular structure called the corpus luteum, which had been known since the seventeenth century.

Such new concerns were not merely histological but represented a broadening of medical and scientific interest beyond the sphere of the secondary sexual characters or the treatment of supposedly feminine complaints into the physiology of the menstrual cycle and that of reproduction. Perhaps the most important late nineteenth-century hypothesis on luteal function was that associated with the names of John Beard (1858–1924) and Auguste Prenant (1861–1927). Beard lectured on embryology and vertebrate comparative anatomy at the University of Edinburgh from 1890 until 1920 but had undergone his higher training in German universities.[139] Beard argued that since ovulation did not occur during pregnancy, it was very likely that the corpus luteum was "a contrivance for ... preventing a normal ovulation".[140] He did not, however, speculate on its means of action. In 1898, Auguste Prenant, professor of histology at Nancy and, later, Paris, proposed a more detailed hypothesis. Although the publication of Knauer's experiments was still in the future, Prenant was better informed than Beard about recent speculations on the internal secretory activities of the ovaries. Emphasizing the histological similarity between the corpus luteum and other glands either suspected or known to be active internal secretors, Prenant speculated that the corpus luteum suppressed ovulation during pregnancy not through the vague nutritive influences adumbrated by Beard but through chemical secretions.[141] This notion came to be known as the Beard-Prenant Hypothesis.[142]

Virtually contemporaneously and in all probability independently, the German embryologist Gustav Born (1851–1900) had also identified the corpus luteum as a possible source of important internal secretions. Born died unexpectedly and his hypothesis was actually investigated experimentally on rabbits by his student, the gynaecologist Ludwig Fraenkel (1870–1951), who was assisted on many of the experiments by another young gynaecologist, Franz Cohn.[143] According to Fraenkel, Born had been struck by the facts that the uterine mucous lining — the endometrium — began to proliferate before the fertilized ovum reached the

uterus and a significant corpus luteum never developed in those animals whose embryos were not implanted in the endometrium. From these observations, Born had inferred that the corpus luteum might secrete "substances which cause the first pregnancy changes in the organism, especially in the uterus and thus enable the egg to nidate in the endometrium".[144]

Although Fraenkel designed many different kinds of experiments to test his mentor's hypothesis, the most illuminating ones relied on removing both ovaries (of experimental animals) after conception (but before implantation of the embryo) or cauterizing the corpus luteum with electricity at the same point in the cycle. In both cases, pregnancy terminated and the uterus atrophied, from which Fraenkel concluded that the fertilized egg could not be implanted in the uterine endometrium if the ovaries were removed after conception. The proliferation of the uterine mucosa and its blood vessels was essential for the implantation of the ovum and this proliferation was induced exclusively by the internal secretions of the corpora lutea.[145] If conception did not occur, then the corpus luteum ended its work after triggering the next bout of menstrual bleeding.[146] Although Fraenkel did not completely rule out internal secretory contributions from other histological elements of the ovary, the corpora lutea, for him, were undoubtedly the most important sources of the ovarian internal secretions.[147] The undeveloped state of the prepubescent female reproductive apparatus as well as the post-menopausal atrophy of it he attributed solely to the absence of the corpora lutea.[148]

Fraenkel's arguments were supported by substantial experimental data but they were far from incontestable. The processes he studied could be interpreted differently and his methods criticized: Josef Halban, for instance, argued that since the uterine mucosa began to proliferate before the corpus luteum had taken shape, it was likely that the change was induced by an unknown internal secretion of the fertilized ovum. Halban's chief, Friedrich Schauta, as well as other researchers felt that Fraenkel's method of extirpating the corpora lutea may well have damaged the ovarian tissue — his experimental results, therefore, could not indubitably be attributed to the absence of the corpora lutea alone.[149]

Fraenkel's work has been explored in great detail by Hans Simmer but a crucial point that Simmer has treated somewhat cavalierly is the clinical context of Fraenkel's research. From a narrowly scientistic perspective, it is, obviously, appropriate to criticize Fraenkel and his interlocutors, as Simmer has done, "for obscuring the issue by clinical observations" and for combining "observations on a menstruating female [presumably a *human* female!] with those obtained from a non-menstruating animal".[150] For the historian, however, the interpenetration of laboratory data with clinical observations in Fraenkel's work is of greater interest. Fraenkel was a busy practising gynaecologist and although his allegiance to science was unquestioning, he was not cauterizing corpora lutea simply to garner physiological data but also to discover ways of improving the practice of clinical gynaecology. He, and the other gynaecologists who debated his work, were not obscuring the issue: for them, the physiology of the corpus luteum was not some

arcane physiological conundrum to be mulled over in a laboratory but a biological issue of great potential relevance to the clinic.

At the very beginning of his comprehensive 1903 report, Fraenkel stated that he was going to present and prove his theory of the physiological functions of the corpus luteum *and* suggest "important practical applications" and the therapeutic relevance of his theory.[151] Later, he asserted that although the human machine was too complicated to guarantee that even an accurate theory would always lead to genuine, lasting improvements in clinical practice, any theory that claimed to explain a range of physiological and pathological processes of the female organism must be tested for its applicability to treatment of real patients.[152] This was, of course, the golden age of organotherapy and Fraenkel acknowledged that for some years, extracts of bovine and porcine ovaries had been available under the proprietary names of Oophorin and Ovariin, the therapeutic effects of which on the symptoms of menopause (whether natural or induced by bilateral oophorectomy) were sometimes impressive but often marginal or downright negligible. It was his work on the corpus luteum that, according to Fraenkel, explained the variability of results with Oophorin: the latter used the entire ovary whereas it was only the corpus luteum that was responsible for menstruation and for maintaining the nutrition of the uterus.[153]

The secret of a more effective ovarian extract, therefore, was to prepare an extract solely from the corpora lutea, which was not particularly difficult in cows, the corpora lutea of which were substantial. (It would, he remarked, be ideal if one could use human corpora lutea but "ethical and other difficulties" prevented that.[154]) He had prepared such an extract from non-pregnant cows (naming it Lutein I) and had used it clinically for more than a year. It had proved to be of enormous use in combating menopausal symptoms such as flushes, anxiety, palpitations and tremors promptly, reliably, virtually without unpleasant side effects and at not too high a price.[155] Except for such more or less subjective menopausal symptoms, however, Lutein had been unpredictable in its effects and Fraenkel suspected that its efficacy was essentially confined to the former. Fraenkel had also prepared a corpus luteum extract from pregnant cows, which he christened Lutein II, that had not shown any noteworthy effects on menopausal symptoms, and while it might eventually prove to be of some use in various unpleasant symptoms associated with pregnancy, no reliable results could yet be reported.[156] Nevertheless, Fraenkel emphasized, one had to admit that while one could induce uterine atrophy and amenorrhoea by removing the corpora lutea, one could not reverse those phenomena by the administration of luteal extract. Lutein could remove many menopausal symptoms but it could not re-establish menstruation itself, nor did it seem to be useful in forestalling threatened abortion.[157] The clinical relevance of Fraenkel's theory of luteal function, however, was not restricted to organotherapy alone. It implied that the recent surgical enthusiasm to perform immediate ovariotomies for serious ovarian tumours in pregnant women needed to be curbed. If the operation simply could not be avoided, then the surgeon should take the greatest care to resect the tumour in such

a way as to leave the corpus luteum intact.[158] It was best, however, to postpone the operation until the pregnancy had reached a stage when the protective function of the corpus luteum was no longer necessary — from the reports of others, this period probably began after the first eight weeks.[159] He also speculated that antibodies could be synthesized against different components of the ovary, which might be used for "total sterilization by biochemical means" of women or for the cure of osteomalacia, which was invariably alleviated by bilateral oophorectomy.[160]

Fraenkel's work was stimulating but not universally accepted by his peers. The role of the corpus luteum as an endocrine structure was not rejected out of hand but each of his contentions about its physiological significance was contested. The reason for this was simple. Many scientists were now beginning to suspect that the ovary was not one gland but a consortium of at least three important kinds of tissue, each, according to different scientists, producing internal secretions of importance for the maintenance of the sexual attributes of femininity: the Grafian follicle and the interstitial cells were the two entities most commonly thought to be responsible for the endocrine actions of the gland. In 1910, Artur Biedl concluded from a fresh examination of Fraenkel's evidence that he had not succeeded in proving that menstruation depended on ovulation and the corpus luteum or that the corpus luteum ensured the nutrition of the uterus from puberty to menopause or that the corpus luteum was necessarily essential to sustain pregnancy, although Biedl was inclined to be rather more impressed by Fraenkel's evidence on this last issue than on others. Nor did Biedl discover much supportive evidence in the literature for Prenant's hypothesis that the corpus luteum inhibited ovulation during pregnancy. All these processes, decided Biedl, "may result, with equal probability, from hyperfunction of the interstitial cells and reduction of activity on the part of the other tissues, particularly of the Graafian follicles".[161] Until the late 1920s, in fact, the role of the corpus luteum would remain controversial and undecided.

But Fraenkel's work shows us a novel version of the ovary: a gland whose secretory activity was periodic — the corpus luteum, of course, was not a constant structure — and located (almost entirely) in certain transient bodies. Like the Knauer-Halban model, however, this new model, too, had a strong clinical dimension, albeit it had not been launched with the sole aim of finding a solution to a distinct clinical problem. No matter from what direction and in what stage, the clinical motivations of glandular research were crucial and inseparable from the pathbreaking laboratory studies that have been documented so admirably by Simmer and other historians of endocrinology. Many of the gynaecologists who investigated ovarian functions in the late nineteenth and early twentieth centuries could stand up and be counted with the most competent physiologists of the time — but they were not simply physiologists, with the possible exception of the early Halban. For the vast majority of this group, physiology, pathology and gynaecological therapeutics were parts of an inseparable whole.

OVARY, GENDER AND PROFESSION: THE TRIANGULAR QUEST OF WILLIAM BLAIR-BELL

Let us now turn to early twentieth-century England for a very different illustration of the diversity of motives and contexts that drove research on the glandular basis of femininity. The Liverpool gynaecologist William Blair-Bell (1871–1936) is usually remembered today only as the first president of the Royal (formerly British) College of Obstetricians and Gynaecologists and a leading figure in the struggle to establish the College, the idea of which was opposed by powerful sections of the medical profession. "We his followers", wrote the gynaecologist V. B. Green-Armytage, "must never forget our debt to his fanatic zeal and dynamic force of intellect, for to him ... we owe our College.... He subordinated everything personal and professional to the attainment of his one ambition — a Royal College of Obstetricians and Gynaecologists".[162] As one of his peers put it more pithily, Bell was "the restless, lovable torch-bearer who never forgot nor allowed anyone else to forget that he was bearing a torch".[163]

The College, however, was not the only torch borne by Bell. He was also indefatigable in crusading for the recognition of gynaecology as a true science and his argument was based on his experimental studies of sex gland function.[164] For Chrobak and Knauer, glandular physiology was subsidiary to glandular therapeutics; for Blair-Bell, on the other hand, the knowledge of sex-gland function was necessary in order to transform the old crafts of gynaecology and obstetrics into one genuine, unified science.[165] "Unfortunately", Bell wrote toward the end of his life,

> for the last hundred years gynaecology has been of little interest to the physiologist, anatomist, and biochemist. So the credit for practically all modern advances have fallen to clinicians. Now, in the immediate present, it appears that ... the scientific aspects of our subject are attracting the attention of specialists in various branches of biology. This is probably as it should be, so long as the practitioner, who hitherto has solved his own difficulties by scientific investigation, is encouraged to take them to the experimentalist for solution.

Amongst examples of recent advances, the first he cited was the progress in understanding of "the hormonopoietic system", and was confident that "historians of the future" would recognize these advances with due honour.[166]

While all previous theories of glandular femininity had focused on the ovaries, Bell looked upon every ductless gland in the female body as of sexual importance. In the Arris and Gale Lecture delivered in 1913 at the Royal College of Surgeons, he proclaimed: "My endeavour will be to emphasise ... the importance of all the internal secretions in the special economy of the female so that those who are interested in gynaecology may come, if they have not already done so, to look upon *all the ductless glands as genital glands.*"[167] Woman, for Bell, had no function apart from the sexual: "the characteristic functions ... of the female", he wrote, "are those associated with the genital activities — menstruation, gestation and parturition and lactation".[168] This was not simply a patriarchal conviction but was also crucial to

Bell's lifelong effort to elevate the status of gynaecology. If woman was wholly and exclusively governed by her biology, then that would be the best justification for the existence and the advancement of the gynaecological profession, which, according to many élite generalists, was an example of the dangerous proliferation of medical specialities.[169]

As Bell rightly pointed out, most medical scientists and clinicians of the time believed that it was the ovary that determined the nature of femininity, although some did concede a certain, usually vaguely defined, role to some of the other ductless glands.[170] "No doubt", Bell asserted, "this diffidence is due partly to the incomplete state of our knowledge; but I think it arises more directly from the isolated way we consider most of the structures of the body. We trace the products of metabolism from organ to organ without taking into consideration the fact of what I may best describe as organic harmony between various structures themselves".[171]

For Bell, femininity was a glandular phenomenon but not a monoglandular one. His best-known work, *The sex complex* (1916), was based on the conviction that "*femininity itself is dependent on all the internal secretions*". The ungainly title of the book, far from indicating any allegiance to psychoanalysis, was Bell's omnibus term for all the sexually active internal secretory organs.[172] The ovaries, of course, were crucial components of this ensemble but their endocrine functions were intimately and complexly linked with those of the others. Bell remained an agnostic about the number or histological source of the ovarian secretion(s), topics that were beginning to concern British physiologists such as Francis Marshall. Bell contented himself with the observation that "the internal secretion or secretions of the ovaries have never been isolated; indeed it is still a matter of dispute as to how and where it is or they are produced".[173]

His doubts did not, however, hold him back from performing experimental removals of ovaries in cats, does and rabbits, some of them pregnant. His findings essentially replicated those of other workers, suggesting that pregnancy could not be maintained in the absence of the ovaries. He then transplanted ovaries, again replicating what others had already found: that a successfully transplanted ovary was perfectly adequate for maintaining the secondary sexual characters. Bell's particular interest, however, was in exploring the role of the ovaries in the general metabolism of the female organism. Removing the ovaries of six cats, he collected their urine for varying periods, and found that calcium excretion was diminished by one-half after the removal of the ovaries. Relating this experimental finding with the clinical observation that osteomalacia was often cured after bilateral oophorectomy, Bell concluded that the ovaries promoted the excretion of calcium.[174] Overall, bilateral oophorectomy lowered metabolism as a whole — and in women, whose metabolism was "much more easily disturbed than that of lower animals", it could also lead to psychoses and neuroses.[175]

Bell was also greatly interested in the effects of oophorectomy on the other ductless glands but his data were far from illuminating. The thyroid, for instance,

seemed to remain unaffected in cats, but in rabbits, oophorectomy led to pronounced thyroid changes of unclear significance.[176] The thymus (in cats) and the adrenals (in cats as well as rabbits) were also enlarged after oophorectomy but again, the physiological significance of this change remained obscure.[177] Bell, presumably frustrated by the situation, then took the opposite approach. What were the metabolic and specifically genital consequences of the removal of the major ductless glands? Some of his findings seemed to be significant: removing the thyroids from cats resulted in uterine atrophy, for instance, and histological features suggested an increase in pituitary secretion.[178] Despite some interesting results, however, the facts available to Bell and those found by him as a researcher were too few and too murky to support any definitive claims on the polyglandular basis of femininity. Since it was the aim of *The sex complex* to establish that femininity was 'produced' by the combined action of all the ductless glands, the book can only be described as an interesting failure, although somewhat ahead of its time in conceptualization. (Not all of Bell's glandular studies, however, were so inconclusive. He had a particular fascination for the pituitary and lamented its relatively low profile in contemporary research.[179] In 1909, he co-authored an important paper claiming that the extract of the posterior part of the pituitary gland produced intense contractions of the uterus.[180]

Bell was not the discoverer of this phenomenon but he played a major role in popularizing pituitrin (as the extract came to be called) to expedite labour. Its real utility, however, was eventually found to be in arresting post-partum haemorrhage.[181]) Bell was no believer in what later generations would exalt as "pure science", nor an upholder of clinical traditions unsupported by research. True research, for him, spanned the laboratory and the clinic. "In the days that have passed — almost to the end of the last century — the clinician", he wrote, "claimed a very large proportion of all the scientific advances made in medicine and surgery. It is only of recent years that others, working as 'professional' scientists — if I may so term them — have invaded the province of gynaecology and obstetrics".[182] Urging younger clinicians to appreciate how deeply the "patience and courage, knowledge, and scientific attitude of mind engendered by research" benefited one's clinical practice, he deplored the growing separation of the 'science' and the 'art' of medicine.[183] "It is not so very long", he warned them, "since gynaecologists were regarded as accoucheurs trying to do surgery, and obstetricians as midwives". The implication was clear: the art (and profession) of gynaecology would founder if the science was neglected, but being a laboratory-bound scientist was not going to achieve much either.[184] This, however, was 1932, and Bell knew that the golden age of the clinician-researcher was over: much of what constituted the science of gynaecology in the earlier decades of the century had now passed into the hands of full-time laboratory scientists. "Clinicians who take their problems to the laboratory and try to solve them there", he admitted, "are now regarded by the professional scientist as amateurs ... the gynaecologist of the present realizes that there will be little or no scientific recognition for him.... Nevertheless, our great and important

subject has been placed on its present scientific foundation by the endeavours of scientifically minded clinicians during the last fifty years".[185]

Blair-Bell's scientific activities have not been entirely ignored by historians, although one doubts whether he would have been flattered by their opinions. Ornella Moscucci, for instance, has acknowledged him as a pioneer of gynaecological endocrinology, while emphasizing that the historian is struck less by the scientific merit of his contributions than by his rearticulation of the "enduring ideology" that the very identity of woman was constituted and dominated by her sexual functions.[186] Questioning whether Bell's work even justifies the label 'scientific', Roy Porter and Lesley Hall have rebuked him for not maintaining an "open mind in the face of contradictory evidence" and for using scientific arguments "to shore up existing prejudices".[187]

Nobody familiar with Bell's views on the nature and purpose of femininity could reject these judgements. They could, however, do with a bit of modulation. As far as his sexual ideology was concerned, Bell could, in fact, be interestingly ambivalent. He declared, for instance, that it was senseless to consider the male as necessarily superior to the female and waxed eloquent on how her intellect was "a source of personal pleasure and pride" to the human female, while stating in the same breath that "it must surely be recognized by all that the male mind and masculine form are suited to the business of life" while "the central motive of a normal woman's existence is the propagation of the species".[188] He thought that bilateral oophorectomy for trivial complaints like menstrual pain was "unscientific and reckless" and yet, considered masturbation to be so harmful that he proudly reported:

> In one case the patient's distress and remorse at her own evil ways, which she found impossible to check, were such that we excised her clitoris and nymphae. This method of treatment may be adopted with excellent results if the right type of case be selected: the girl who is not suffering with excessive sexuality, but, rather, with the fascination of a bad but pleasant habit, to the detriment of her moral and physical equilibrium.[189]

And yet, he added in the same article:

> A woman is not judged by the standard of masculine sexuality. The average man is supposed to be immoral, and undoubtedly he is. A woman, if she have the same feelings, as is often the case, either becomes ostracized or may suffer from the restraint imposed. Social exigencies, in fact, establish the relative standards which suit the community best, if not the individual.[190]

He berated the "modern woman" for her "rejection of maternal functions", wondering, however, whether the rejection was part of "Nature's plan for securing the disappearance of Man to ensure further evolution".[191] Later, he pleaded with "normal" women to fulfil their reproductive obligations, "for it is the normal woman alone who should perpetuate the race and maintain the dominance of home life, without which men are handicapped both mentally, physically and as citizens". This

is the point where his sexual ideology merged most seamlessly with his professional agenda: "If this be accepted", he went on, "the scope for gynaecology and obstetrics of the highest type is wider than it has ever been. We should aim not only at the best ways of dealing with the abnormalities of the genital system, but also throw ourselves whole-heartedly into the task of encouraging women to maintain a normal psychological outlook in regard to the special attributes and functions of their sex".[192]

"The language and practice of gynaecology", Chris Lawrence has written, "demonstrated to Victorians, on a day-to-day basis, the enormous determining power of the female reproductive parts. From this determinism flowed naturalistic prescriptions which defined the role of middle-class women in Victorian society".[193] The post-Victorian Bell's ideas on female nature and appropriate feminine conduct differed little in essence from those of his Victorian forebears. The discourse of glands, however, enabled him to frame his 'prescriptions' in the language of new, authoritative science. His somewhat confused views on femininity were integral to his larger professional project. To see women as sexual beings and machines for reproduction was also to see them as the *raison d'être* of his own profession. The gynaecologist was not just a medical specialist: he served the human species by ensuring the safety of the vessels through which it was perpetuated. And the reproductive functions of women could be understood and kept in good condition only by proper attention to their ductless glands. We must, therefore, see Bell's glandular research, sexual ideology, and professional vision as different aspects of the same project to elevate the status of gynaecology to glorious new heights. Bell's work illustrates the diversity of intentions that drove gynaecological endocrine research in the early twentieth century. Just as there was no specific discipline of endocrinology, there was no one, monolithic, universally shared approach to the study of ductless glands even amongst gynaecologists.

(MORE) TALES FROM THE VIENNA LABS: THE OVARY AS FOUNTAIN OF YOUTH

So far, we have analysed different hypotheses regarding the functions of the ovary and the possible clinical — particularly gynaecological — relevance of those functions. Gynaecologists were undoubtedly preeminent in ovarian research around the turn of the century but they were not the only medical scientists concerned with the biology of the ovary and its clinical significance. Let us turn our attention, then, to an early twentieth-century physiologist and return to Vienna, the city associated for ever — thanks to Knauer, Halban and not least, the historical efforts of George Washington Corner — with the history of ovarian endocrinology.

Our protagonist — once tipped confidently for the Nobel Prize but consigned to exile and oblivion even before his death — is the physiologist Eugen Steinach (1861–1944).[194] After medical training in Vienna, Steinach began a research career in physiology in the German university in Prague. His first few projects there were studies of neurophysiology that he himself later dismissed as slight. His most memorable experimental work came after he had returned to Vienna as the director

of the physiological section of a private, well-funded research institution. In the city of Krafft-Ebing, Freud and Halban (not to mention Otto Weininger and Egon Schiele), Steinach discovered sex and remained with it for the rest of his career. His work on rats and guinea-pigs reflected as well as participated in the fundamental shift from neural to chemical theories of sexuality. The reason Steinach made headlines, however, was because he displayed far greater alacrity than other leading physiologists in applying his experimental findings to human beings. Experimental biologists, of course, have thought in pretty sweeping, almost triumphalist terms before Steinach — one has to think only of Jacques Loeb — but Steinach did not confine his grandiose style to his laboratory. His arcane laboratory research, he promised, had discovered a grand prize available to all of humanity for the price of a simple operation. To use the term he himself used and then never ceased to regret, he promised rejuvenation.

Most of Steinach's early research addressed the sexual development of rats and guinea pigs. His approach to this topic was shaped by the admonition of the embryologist Wilhelm Roux that biological development should be investigated, not by studying normal processes, but by artificially inducing abnormalities and monstrosities, and deducing therefrom the course of normal development. In order to investigate the development of the male and female sexual characters, then, a properly Rouxian way of proceeding would be to to distort, impede or modify their development — the resulting abnormalities would reveal how normal sexual development occurred. One relatively simple way of doing this was by castration of a prepubertal animal's own sex glands, whether female or male, and replacing them with the glands of the other sex. Another, more complex procedure would be to castrate prepubertal animals and then implant the sex glands of both sexes.[195] Steinach attempted both kinds of experiment, obtaining intriguing results that were widely discussed and debated in the contemporary medical literature.[196] He was nominated for the Nobel Prize in Physiology six times between 1921 and 1938, although he was never to receive it.[197]

In his first two series of experiments on rats, reported in 1894 and 1910, Steinach established that somatic and behavioural sexual maturity was induced by chemical substances from the sex glands.[198] He proceeded to show that the sexually active internal secretions of the testes were produced by the interstitial cells, which had nothing to do with the production of spermatozoa, a hypothesis that generated great controversy at the time, although it was eventually accepted as correct.[199] He then showed that male or female development in rats and guinea-pigs were not, as some leading scientists had argued, programmed *ab ovo*, with the sex glands merely exerting a protective or stimulatory function over them.[200] When ovaries were grafted in male rats castrated in infancy, the male sexual charaters of his grafted castrates were severely stunted, and the sexual characters that were normally 'indifferent' in males, such as breasts and nipples, underwent remarkable growth, leading even to lactation.[201] Steinach then tried to create hermaphroditic animals by transplanting both ovaries and testes in male guinea-pigs castrated in infancy.

When both grafts survived, these animals developed a typically male body-build, but their mammary glands were fully feminized. Behaviourally, the animals with surviving dual grafts were cyclically masculine and feminine.[202] Although the notion of genetic determination of sex was beginning to be accepted at the time, Steinach never mentioned it in any of his published writings. In one private letter, however, he conceded that sex *was* indeed determined by genetic factors but insisted nevertheless that — and this, for him, was crucial — the *characteristics* of sex could always be modified by modulating the functions of the sex glands.[203] Above all, Steinach emphasized repeatedly, it was crucial to appreciate that the sex glands stimulated those characters associated with its own sex and actively inhibited those associated with the other. The sex glands, in short, were "antagonistic" in their functions.[204]

Steinach was convinced that his experimental findings were clearly applicable to humans and suggested that hermaphroditism and homosexuality were ultimately due to lack of sexual differentiation in the gonads, which caused the production of both male and female secretions.[205] This hypothesis, shaped significantly by his communications with the contemporary clinical sexologist and homosexual emancipationist Magnus Hirschfeld, led Steinach to introduce a highly controversial — and rapidly discredited — 'treatment' for homosexual men, in which one testicle was removed and replaced with a testicle from a heterosexual donor.[206] A thorough analysis of this episode being available elsewhere, I should emphasize here only that in the purely intellectual sense, Steinach's homosexuality-cure represented a logical outcome of his "analytic" experiments on the sex glands — it proved ineffective, to be sure, and was significantly influenced by contemporary cultural politics, but it was not the bizarre piece of quackery it might seem at first glance.

Steinach's involvement with homosexuality was but one result of his experimental research on the development of sex. For some time, he had also been investigating changes in sex-gland function during senility: Wilhelm Roux, after all, had pointed out many times that the study of biological development should include not simply the analysis of the origin and maintenance of organic forms, but also of their involution. Steinach began his research on ageing and involution with the proposition that the somatic sexual characters (most importantly, the seminal vesicles) were present at all ages but developed fully only after puberty under the influence of the sex-glands. In rats castrated before puberty, the seminal vesicles remained undeveloped in adulthood. In intact animals, too, the somatic sexual characters regressed to a near-infantile condition in old age. In children and in aged humans, the differences of gender were blurred and "everything that is typically male or female becomes colourless and indistinct. Just as it is often difficult to distinguish between the face of a little girl and that of a little boy, so the shaven face of an old man resembles that of an old woman.... And naturally temperament and disposition begin to lose their typical expression for the different sexes, the old man revealing only traces of his former masculine aggressiveness and the old woman but feeble remnants of her modesty and forbearance".[207] Senility,

then, was a desexing process.

Therefore, Steinach reasoned, the senile were functionally analogous to pre-pubertal castrates: both lacked adequately functioning sex-glands as indicated by the diminished size of the seminal vesicles.[208] This was the crucial analogy that led to his rejuvenation technique — if senility was akin to the consequences of castration, then any therapeutic intervention that ameliorated the latter might also have significant effects on the former. He issued a preliminary communication on the subject in 1912. As we saw earlier, he believed that it was the interstitial cells of the testes that produced the internal secretions of the gland — therefore, if one could induce the proliferation of those cells, then the secretory deficits of senility ought to be compensated. Theoretically, one could provide the interstitial cells with the space to proliferate if one could destroy the germinal cells. Since it was well-known that ligation of the vas deferens caused germinal atrophy, Steinach attempted to rejuvenate senile male rats by bilateral vasectomy.[209] Within a few weeks of the operation, he reported, the previously lethargic, underweight, and almost lifeless rats had become active, gained weight, developed a glossy new fur, and regained sexual interest. The seminal vesicles, too, had regained their former dimensions. His brief report ended with a promise to explore the applicability of this technique to humans.[210]

Steinach's monograph-length paper on rejuvenation, dedicated to Wilhelm Roux on the occasion of his 75th birthday, was published in Roux's own journal in 1920 and caused an immediate sensation because it incorporated the case-histories of three human subjects of the operation. The vasectomies had been performed at Steinach's request by his associate, the urologist Robert Lichtenstern, during operations for hydrocele or testicular abscess. Steinach reported that the men, who had not been aware of the additional operation, responded as markedly to it as had the rats.[211] The very first patient was a coachman, only 44 years of age, but presenting "a typical picture of premature senility without organic disease". He had lately been unable to work for long hours and had lost weight and appetite. His skin was dull, his hair grey and scanty, and his muscles weak. The operation was performed under local anaesthesia on 1 November 1918, but there were no dramatic consequences for the first three months. Then gradually, his appetite increased, he gained weight and his appearance became hale and hearty; a year later, his hair had grown thicker and he reported that he now carried "loads up to 220 pounds with ease". Eighteen months after the operation, the patient "with his smooth, unwrinkled face, his smart and upright bearing" looked like "a youthful man at the height of his vitality".[212] Soon, reports of similar cases of successful rejuvenation by the Steinach Operation were pouring in from all over the world.[213] The *New York Times* reported in 1923 that every major American and European city already had a number of surgeons specializing in the operation.[214] Writing in 1940, Steinach himself referred to more than two thousand operations performed by surgeons in Vienna, New York, London, St Petersburg, Copenhagen, Chile, Cuba, and India.[215]

What, however, was the physiological basis of rejuvenation? In old age, Steinach argued, the tissues and organs did not themselves degenerate but functioned poorly because of undernutrition.[216] With improvement of blood circulation, the 'senile' tissues could regain most of their functions. The secretions of the sex glands, Steinach claimed, did not just exercise sexual effects but also revitalized the entire organism by improving blood circulation. His follower, the surgeon Peter Schmidt, speculated that the sex gland secretions reduced the tension of the blood vessels by reducing the irritability of the sympathetic nervous system, thus leading to rapid improvement in circulation.[217] Many other explanations were also offered.[218] But so little was known about the nature and physiological actions of the sex-gland secretions that no detailed explanation of the rejuvenative effects of the Steinach operation was conceivable. An almost mystical faith in the powers of the sex glands hung over all speculations. "Sex", declared Steinach, "is the root of life. Just as it produces physical and psychic maturity, induces and preserves the period of flowering, shorter or longer, here richer, there poorer, so it is also responsible for the withering of the body and gradual loss of vitality. Sex is therefore the obvious means for natural stimulation or 'activation' in youth, and also the instrument for methodical 'reactivation' in old age. Sex is not only the measure for the rise, peak, and fall of the currents of life, but also, up to a point, for their restitution".[219]

Medical and popular interest in rejuvenation was predominantly related to men. The rejuvenation of ageing women was far from neglected, but since females had no vas deferens to ligate, and ovarian transplantation, although theoretically likely to yield similar results, was too serious an operation, even Steinach could not easily devise a relatively simple surgical method of stimulating the ovary. He did, however, explore other methods with characteristic adventurousness, but curiously, did not publish his results a fraction as extensively as he did with his male rejuvenation cases. Steinach's personal papers do not seem to have survived their confiscation by the Nazis in 1938 but in his correspondence with the New York doctor Harry Benjamin, there is ample evidence to suggest that contrary to the impression given by his publications, Steinach was experimenting with various, apparently promising methods of female rejuvenation in the 1920s. Roughly speaking, there were two overlapping phases in Steinach's research on the rejuvenation of women. During the first phase, he concentrated on finding a way of stimulating the ovaries to secrete greater quantities of hormones. In the second, he used an ovarian extract named Progynon that he had himself developed in collaboration with the pharmaceutical firm Schering-Kahlbaum, supplementing it with some of the procedures to stimulate the ovary that he had evolved during his first phase of research.

That first phase had begun quite early, with an investigation conducted with Guido Holzknecht, a pioneering radiologist, around 1913.[220] The premise of the experiments was identical to that of Steinach's rejuvenation technique for men: if the secretory elements of the sex glands (as opposed to the germinal elements, which produced the gametes) could be induced to proliferate beyond their usual mass, then their influence on the sex characters would be enhanced. Steinach had

noticed in his previous experiments that female castrates, for instance, implanted with testes became, in some respects, *more* physically imposing than intact males from the same strain. A feminized castrate, conversely, became *more* feminine in build and appearance than a female of the same strain. The explanation, Steinach suggested, was that in the transplanted gland, the secretory cells hyperproliferated and secreted greater quantities of sex hormones than in normal cases.

Steinach and Holzknecht aimed to discover whether this hyperproliferation could be induced in intact animals and used x-rays for the purpose.[221] 2–4-month-old female guinea pigs, held still in a special, snug cage, were irradiated from the back. The aim was to destroy the more radiosensitive germinal elements of the gland, while leaving the somewhat hardier secretory elements undamaged and free to proliferate. The dose and direction of radiation were crucial: they obtained the best results from a moderate dose directed from the back: higher doses or rays directed from the front destroyed both types of cells, resulting essentially in complete castration.[222] The results were slow to appear and were positive in only about 40% of irradiated animals: after 3-4 weeks, the hair fell out from the back and the breasts and nipples began to enlarge, eventually secreting milk for a period of 2-3 weeks. Simultaneously, there was a diminution in general skeletal growth, compared to normal females of the same age. In autopsy, the ovaries were small, the uterus and breasts hypertrophied and hyperaemic. Histologically, the ovary was full of necrosed follicular tissue and packed with hypertrophied secretory cells.[223]

Surprisingly, in his published report on rejuvenation, Steinach did not allude to the possibility of using this method to rejuvenate females. In the few pages devoted to the possibility of rejuvenating females, he described experiments with ovarian transplantation. Otherwise, the entire report was devoted to males. His friend and disciple Harry Benjamin published more on using x-rays to rejuvenate women and regularly prodded Steinach for information and insights on rejuvenating women, not only from scientific interest (although that was strong enough) but also, as he stressed in a 1922 letter, because the matter was extraordinarily important from the financial point of view.[224] Benjamin was indefatigable in his attempts to 'sell' Steinach in the New World and soon came to be known as the leading expert on the Steinach method in America. In one early article, he emphasized that the effect of x-rays on bodily organs (and especially endocrine glands) was not necessarily destructive. In low doses, they could exert a stimulatory effect, especially on the female organism. This, indeed, was the belief of some Central European experts, although it was contested by other authorities, including, powerfully, by Holzknecht himself.[225] Benjamin, however, had no serious doubts about the stimulatory effects of small doses of radiation. He believed that it was not even entirely accurate to assume, as had Steinach and Holzknecht, that in order to stimulate the ovarian secretory cells, it was necessary to destroy the germinal tissue. Weak irradiation, repeated several times, was enough to stimulate ovarian secretion without causing sterility: large doses, in general, did not work anyway where small doses had failed.[226]

"For the retardation of age", he wrote, "I, myself, have treated over one hundred patients showing signs of a congenital or acquired hypofunction of the ovaries.... I have used from three to six x-ray treatments, seven to ten days apart, never applying more but often much less than one quarter of the so-called castration dose at one time".[227] The majority of his patients, Benjamin explained, were around the menopausal age, some considerably older.[228] Most of them complained of symptoms typically associated with menopause (such as hot flushes and sweats) and those that Steinach had identified as the characteristic symptoms of "premature senility": lack of physical and mental energy, weak memory and a general falling-off in those skills that "were most vital to the individual in question".[229] In his first reported case, Benjamin dealt with a 64-year-old woman, who "for several years had complained of an increasing lack of mental activity". She found it hard to concentrate and felt her imaginative ability had declined. No physical disorder could be discovered and Benjamin diagnosed her as a case of "mental sterility". In February 1922, he began to treat her with x-rays: first, a "stimulation dose" was directed at the right ovary and three more treatments were administered at weekly intervals, alternating between right and left ovary. After six weeks of the first dose, "this very intelligent patient, not hysterical at all, stated that she slept better ... that she felt her brain clearer: that she had begun to do considerably more work, and that she felt more 'sustained' in it. New ideas had come to her 'like a flash', as rarely before".[230] In June 1922, she received another two treatments. When examined in August, she reported that "'nothing could tire her any more'".[231]

This patient was the novelist Gertrude Atherton (1857–1948). In her autobiography, Atherton recalled that when she arrived in New York in the spring of 1922, her "mental dynamo refused to tune up" and she simply couldn't find an idea for her next novel. Then, one morning, she read a newspaper feature on Steinach's rejuvenation technique, which quoted Harry Benjamin as having said that "women were running to the Steinach clinic from all over Europe, among them Russian princesses who sold their jewels to pay for treatments ... that might restore their exhausted energies and enable them to make a living after the jewels had given out".[232] She now thought she had the idea for her next novel and went to see Benjamin, seeking information on the subject. "Then when I told him of my period of mental sterility, which had lasted for over a year, and of my dissatisfaction with the preceding books, he asked me why I did not consider taking the Steinach treatment.... I was always ready for something new and made up my mind then and there."[233] Thus she found herself to be Benjamin's first known female patient — alluded to as "that writer" in his letter to Steinach of April 1922. She found the treatment to be "a painless and rather boring process" and for a month during it, her "brain was torpid.... When capable of thought I wondered if I were ruined for life". And then all of a sudden, she "had the abrupt sensation of a black cloud lifting from my brain, hovering for a moment, rolling away. Torpor vanished. My brain seemed sparkling with light.... I almost flung myself at my desk. I wrote steadily for four hours.... It all gushed out like a geyser that had been 'capped' down in the cellars

of my mind, battling for release. That geyser never paused in its outpourings until the book was finished, five months later".[234] This was her rejuvenation novel (*Black oxen*, 1923), and it turned out to be the biggest success of her career. Throughout her life, she remained a staunch believer in Steinach and his theories and made no secret of her own recourse to rejuvenation.[235] Atherton certainly did Steinach and Benjamin proud: parts of her novel read like publicity tracts written by them. The author was swamped with letters from readers asking for information on the Steinach technique. "Poor Dr. Benjamin!", she recalled.

> I nearly ruined him. Women besieged him, imploring him to give them the treatment free of charge or at a minimum price. It was the first time they had seen a ray of light in a future menaced with utter fatigue and the clutching of younger hands at the jobs that were wearing them out. He was too kind and conscientious to deny the most appealing cases, and they must have taken a good deal of his valuable time and left him out of pocket, for somebody had to pay the laboratory expenses. But he was rewarded, for his fame spread. I met several of those patients with whom the treatment had been as successful as with me. I also had enthusiastic letters from others who, living abroad, had gone to Steinach's clinic or to Dr. Schmidt in Berlin.[236]

Later, Atherton was widely ridiculed in the German and American press for her impulsive suggestion that Germans should rejuvenate their ageing élite and thereby gain a leading place in the world once again.[237] Although Atherton was being overly optimistic about the number of patients she referred to Benjamin and Steinach — the correspondence is full of complaints about the scarcity of patients! — she rendered them an invaluable service in publicizing their work, especially with women. Even the secretive and mercurial Steinach — who often craved publicity but reacted furiously at anything that struck him as even remotely *unwissenschaftlich* — was appreciative of her efforts, especially the patients she sent to him in Vienna. As he changed his techniques, he offered them (sometimes *gratis*) to Atherton until 1937.[238]

Techniques for rejuvenation of women, indeed, changed far more over Steinach's career than they did for men. Radiation soon began to be combined with diathermy (i.e. warming the whole body and the ovarian regions electrically), the rationale being that the function of the ovary was stimulated by the heating in itself and, when x-rays were administered afterwards, the gland was more radiosensitive and responded to a smaller dose of x-rays than would otherwise be the case.[239] Benjamin also began to irradiate the other endocrine glands concomitantly, especially the pituitary and the thyroid.[240] How popular were these rejuvenative treatments? Numbers, of course, are unavailable — largely because of the loss of crucial archival records, especially Steinach's own papers — but purely impressionistically, it is hard to resist the suspicion that in spite of enthusiastic statements in private as well as public, rejuvenation for women was less of a draw than the Steinach operation for men. Steinach's disciple Peter Schmidt said almost as much: "Rejuvenation in

women demands the expenditure of much more time than in the case of men. It is also much more complicated, and needs considerable modification to suit each individual case, whereas the rejuvenation of males by the Steinach operation is an almost invariable procedure."[241]

Moreover, it was thought that female rejuvenation could not be successful if attempted too late in life. "Eve cannot afford to wait so long for her rejuvenation as Adam", observed the German-American journalist George Sylvester Viereck (1884–1962), a great champion of Steinach in the popular arena. "Men of seventy and over have been successfully Steinached. In women of so mature an age, the attempt would be almost hopeless. The most favorable time is the period immediately before, during or shortly after the change of life." Doctors agreed. One clinician who was not entirely sceptical about the Steinach treatment wrote in 1939 that the results of ovarian irradiation were "most problematical, since by the time a woman applies for rejuvenation therapy most of the ovarian tissue has been atrophied and there is nothing left to reactivate".[242]

Technical difficulties and biological uncertainties, however important, may not have been the sole reason for the lack of popularity of rejuvenative procedures for women. Men, according to most practitioners, hoped (indeed, expected) their efficiency, sexual desire and vitality to be reawakened after the Steinach operation but women, apparently, hoped to regain beauty. Peter Schmidt, for example, declared: "For the woman of modern times, the loss of her good looks entails serious forfeits in occupational or social life ... the reactivation of a climacteric woman may re-establish menstruation, may lower blood pressure, may improve functional capacity in various ways, but she will be inclined to say 'thank you for nothing' unless at the same time you have improved her looks by effacing the signs of age in the skin of her face."[243] Schmidt's assessment may not, of course, have been an accurate reflection of female attitudes to rejuvenation, although one notes that Gertrude Atherton's rejuvenated heroine regained not only vitality and sensuousness but also her youthful beauty. But even if this point is inaccurate in itself, it suggests that because of their conviction that men and women sought different things from rejuvenation, doctors sought to rekindle not youthful vitality in a global sense but only *those* attributes of vitality that were culturally appropriate to the client's gender — efficiency and strength for men, beauty and sex appeal for women.[244] Cultural values and expectations, in short, were integral to the science and practice of rejuvenation.

The full story of Steinach's attempts to rejuvenate women must be left for another occasion; what is of moment here is to note the distinctiveness with which Steinach harnessed the ovaries in his service. The endocrine functions of the ovary were paramount here, as in most of the other episodes we have discussed. The reproductive functions, however, were not simply ignored but targeted for active elimination in order to convert the ovary into an exclusively endocrine organ. (In men, too, the Steinach operation for rejuvenation was designed, of course, to achieve exactly this.) Once liberated from their reproductive obligations, the sex

glands would turn into fountains of youth and vitality. "Instead of giving life to children", Benjamin once wrote regarding the operation for males, "aging men were to be made to give life to themselves".[245] For females, too, life and vitality were available only at the cost of forgoing procreation, but for a female to choose vitality over motherhood during her reproductive years was obviously not an easy choice in cultural terms. Nor, indeed, was society of the time likely to approve of a post-menopausal woman's desire to reawaken sensuous femininity (which rejuvenation was widely supposed to induce) with the aid of science. The heroine of Gertrude Atherton's novel *Black oxen*, the 58-year-old — but, after rejuvenation by ovarian irradiation, apparently thirtyish — Countess Zattiany (i.e. the rejuvenated Mary Ogden), declares proudly to her new 34-year-old lover Clavering, "I do not merely *look* young again, *I am* young. I am not the years I have passed in this world, I am the age of the rejuvenated glands of my body". She immediately adds, however: "Of course I cannot have children. The treatment is identical with that for sterilization. This consideration may influence you."[246]

Much broader cultural forces shaped Steinach's entire rejuvenation programme, whether for males or females, and the ways in which it was perceived by the medical profession, the intelligentsia, or the media. The space at my disposal does not permit an extended discussion but four cultural contexts need to be emphasized here — which, obviously, were not self-sufficient in generating Steinach's research programme but acted in concert with his intellectual and professional contexts, among which the precepts of Wilhelm Roux must be counted as the most crucial. Of the cultural contexts, one crucial to Steinach's work was the cult of youth in early twentieth-century Central Europe. Youth, as Heiko Stoff has pointed out, did not simply signify a particular stage of the life cycle but symbolized purity, naturalness, health and beauty — it was the symbol and goal of the New Man. Healthy youth was, crucially, sexually differentiated: men were virile and women were feminine.[247] This insistence on health being incompatible with incomplete sexual differentiation was, of course, a recurring motif in Central European cultural history since, at least, the late nineteenth century. In the heyday of degeneration theory, one of the cardinal features of degenerate bodies was their lack of adequate gender differentiation: as Barbara Spackman has remarked, degeneration was "degenderation".[248] An effeminate man or a masculine woman was biologically degenerate. At the turn of the century, in Steinach's Vienna, the philosopher Otto Weininger (1880–1903) published his massive work, *Geschlecht und Charakter* (*Sex and character*) in which he condemned his epoch as "the most feminine of ages", while Weininger's admiring reader, the satirist Karl Kraus called it, simply, "the vaginal epoch". Much of the terror of progressive effeminacy of men and virilization of women — which, indeed, amounted to a whole crisis of defining 'male' and 'female' — had been triggered by the emergence of feminist activism in the late nineteenth century and Steinach's work helped assuage such anxieties by implying that even if the ideal world of 100% men and 100% women didn't exist, medicine was now capable of inventing it.[249] This makes the interpretation

of Steinach's concept of sex-gland antagonism rather more complex than it might seem at first glance. Total sexual differentiation was a norm, an ideal that might, perhaps, have been a feature of the misty past but it certainly was not a reality that turn-of-the-century thinkers and doctors could point to. The majority of doctors and cultural critics addressing issues of sexuality and gender not merely acknowledged but often worried over the sheer fluidity of gender that they perceived in bodies, minds, lives and broader culture.[250] Whether they attributed it to degeneration, congenital developmental anomalies or incomplete evolution of the human species, the phenomenon was virtually unquestioned. Almost equally unquestioned was the *desirability* of full differentiation between men and women.[251]

It is this mismatch between reality and norm that Steinach's work claimed to resolve. Male and female sex glands were fully antagonistic — in theory and as norm — but they often failed to translate that antagonism in individuals. But now, thanks to biological expertise, gender would be clear and unambiguous in utopia: it was, in fact, ever sharper gender differentiation that would *create* utopia. Small wonder, then, that Karl Kraus, who hated suffragettes and journalists as the symbols of his "vaginal era", imagined Steinach saving the world by changing feminist activists into maternal women and journalists into real men, or that Magnus Hirschfeld, the homosexual emancipationist who believed that male homosexuals were biologically feminine rather than degenerate or debauched, lionized the Viennese wizard or that Steinach's wife claimed that her husband's research had validated the speculative theories of Otto Weininger![252]

Anxieties on gender had, if anything, been further sharpened by the loss of young male lives in the Great War and what seemed then to be the destruction of Germanic civilization itself. Post-war conditions in Central Europe stimulated the development of what Paul Weindling has termed "regenerationist biology", a cluster of beliefs, practices and trends that sought the revivification of society by biological means, of which eugenics was only the most prominent strand. Steinach's work was eminently compatible with this trend. As his colleague and supporter Paul Kammerer — socialist, fervent Lamarckian and passionate believer in biological solutions to social problems — suggested, rejuvenation should be applied particularly to men who could guide humanity to a higher plane.[253] The journalist George Sylvester Viereck observed that "if we save valuable human material by applying the process of rejuvenation impartially to men and women of ripe experience, our dreams of Utopia may come true at last.... Within ever expanding limits, biochemistry will hereafter direct the trend of eugenics and evolution".[254] As Spengler and his epigones lamented the decline of the West, biologists dreamed of creating a new world, a new species, and a new, clearly sexed body throbbing with the energy and spirit of youth. The significance of Steinach's rejuvenation technique within that utopian project far transcended questions of its actual clinical efficacy.

"THE VERY ESSENCE OF EVE": THE NEW EXTRACTS AND THE DISAPPEARANCE OF THE OVARY

Steinach's last major project was the development of a potent ovarian extract, of which we have heard briefly with regard to rejuvenation. The history of this product, which was marketed by Schering-Kahlbaum, and the contributions of the pharmaceutical industry to its development and propagation, are topics that cannot be dealt with here.[255] Nor need we address the exciting, complex story of the development of standardized ovarian extracts that began in the late 1920s. Of course, interesting and illuminating research on ovarian extracts had been reported before that period: even if we ignore the first age of organotherapy inaugurated by Brown-Séquard and his followers, we can, with hindsight, see that some extracts produced in the early twentieth century contained hormones in effective quantities. George Corner identified three gynaecologists as having obtained potent ovarian extracts: Henri Iscovesco in Paris, Ottfried Otto Fellner in Vienna, and Edmund Herrmann, also in Vienna.[256] The implications of this research, however, took a while to make clear sense. To repeat Corner's recollection, "at the beginning of 1923, before Allen and Doisy's first paper appeared, the literature of ovarian endocrinology was in a very confused state".[257] The key to their research, however, was more chemical than physiological or clinical: they succeeded largely because of their intelligent use of solvents. Since the ovarian hormones are lipid-soluble, their use of appropriate solvents led to extracts the chemical composition of which remained mysterious but whose physiological effects could be demonstrated quite clearly in experimental animals.

Edgar Allen and Edward Doisy's identification of an ovarian hormone in follicular fluid led to a glorious new epoch in endocrinology and it is justly celebrated as a classic contribution to the history of the subject. The American duo, a zoologist and a biochemist, as no less an expert/participant/historian than George Corner put it, "did not know enough about the earlier history of the subject to be confused by it".[258] Perhaps more importantly, as Jochen Süß has shown, Allen and Doisy were aided by the emergence of Stockard and Papanicolaou's new, simple and reliable way of detecting oestrus in an animal from smears of its vaginal cells.[259] If one wanted to be perverse, one could even argue that Allen and Doisy's work should be regarded merely as a brilliant, imaginative and topical extension of Stockard and Papanicolaou's revolutionary cytological research. If they had chosen to assess the potency of their extract from traditional indicators like its effects on the growth of the uterus, the outcome of their study may have been very different. The new test was quick, reliable and did not require the animal to be killed and dissected. As Corner pointed out, the physiologist Francis Marshall had also used oestrus induction as a yardstick for extract potency but, using bitches and not having the Stockard-Papanicolaou method at his disposal, Marshall had to "watch for the slow onset of ill-defined signs of oestrus in the bitch" while Allen, using rats and the smear technique, "could read the results of his tests in a day or two". Soon, the test was so precise that Allen and Doisy could define one rat-unit of their hormone as the smallest amount of the extract that could induce cytological changes characteristic

of oestrus in the vagina of a spayed female rat.[260]

Animals, in short, were still important, but their glands now began to lose their hitherto paramount importance as the objects of research. Preparing the extract and testing its potency were the new challenges: the 'whole-gland' approach was almost a thing of the past. While animals had once been used to *produce* knowledge, now they increasingly became devices to test the potency of extracts. As Harry Benjamin wrote sadly to his ageing mentor (in exile in Switzerland after the Nazi takeover of Austria) after failing repeatedly to find him a post in an American research institution, "Hormone research in America is no longer in the hands of physiologists and biologists but almost exclusively in those of chemists ... no Steinach is needed for whatever animal experimentation is required in that research".[261] As more and more came to be known of the chemical nature of ovarian hormones and their actions, however, the gland itself retreated into the realms of histological or pure physiological discourse. Even a dyed-in-the-wool physiologist like Steinach gave up experimenting with new methods to stimulate the ovary once he had developed a satisfactory extract, which a lay admirer of his called "the very essence of Eve".[262]

The chaotic, unregulated interplay of biological research and clinical applications — both shaped only too frequently by cultural forces and imperatives — that characterized the period we have discussed was now a feature of the past, a past that endocrinologists like Herbert Evans would eventually regard as profoundly damaging to the scientific pedigree of their discipline. The neural node of the nineteenth century was now, quite indubitably, a chemical factory — apart, of course, from being the source of ova — and its hormonal products, now that they were available in relatively pure and reliable forms, moved the spotlights away from the factory itself. (But not, of course, all of them. Dorothy Price and Carl Moore's research, published in the early 1930s, on the nexus of the pituitary and the ovary, for instance, integrated the ovary tightly, demonstrably and physiologically into the "endocrine orchestra".[263] But such research was no longer conceived with clinical motivations, nor, of course, did they make headlines in the *New York Times*. As the ovary became fully modern, the gland retreated into laboratories far more distant from the clinic than the laboratories of, say, Knauer or Fraenkel or Steinach.) Glandular physiology, no doubt, went from strength to strength in its own domain, but unlike in the days of Steinach, physiologists now rarely left their own shores to conquer new territory and the land of physiology was no longer the happy hunting ground of migrants and clinical adventurers. Beyond the world of professional physiologists, the ovary emulated the Cheshire Cat, vanishing gently from view, leaving its secretions to entrance a new generation of doctors and patients as powerfully as it had itself once fascinated their forebears.

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Abbreviations:

AfG *Archiv für Gynäkologie*

CfG *Centralblatt [Zentralblatt] für Gynäkologie*

1. See E. Sharpey-Schafer, "Endocrine physiology" (The First John Mallet Purser Lecture, Trinity College, Dublin, 26 June 1931), *Irish journal of medical science*, 6th ser, no. 69 (September 1931), 483–505, p. 484. On Schäfer's career, see Merriley Borell, "Setting the standards for a new science: Edward Schäfer and endocrinology", *Medical history*, xxii (1978), 282–90.
2. E. A. Schäfer, "Address in physiology on internal secretions", *Lancet*, 10 August 1895, 321–4, p. 321; *idem*, "The hormones which are contained in animal extracts: Their physiological effects", *Pharmaceutical journal*, lxxix (1907), 670–74, p. 670; and Merriley Borell, "Organotherapy and the emergence of reproductive endocrinology", *Journal of the history of biology*, xviii (1985), 1–30, pp. 12–13.
3. For a survey of the pre-nineteenth-century history of the ovary — which, if anything, was even more complex than the one I focus on here — see Clara Pinto-Correia, *The ovary of Eve: Egg and sperm and preformation* (Chicago, 1997).
4. In addressing these issues, I do my best to avoid the epistemological assumption that physiological models and clinical practices change because of the correction of earlier mistakes and misconceptions through better, steadily incremental research. This is not necessarily wrong but of little use to a historian exploring scientific issues in their specific, historical contexts. Instead of assessing the achievements of past scientists by the criteria of today's medical science — a trait characterizing the work, for instance, of Hans Simmer and his students at the University of Erlangen-Nürnberg on nineteenth-century investigations of ovarian function, which is otherwise among the most comprehensive and historically accurate work ever done on the history of medicine — I concentrate on trying to explain why certain theories and practices may have been prevalent or obsolescent at a particular time and locale.
5. I shall be using the terms 'gonad(s)' and 'sex gland(s)' interchangeably, although, strictly

speaking, the term 'gonad' means an organ producing reproductive cells alone. For an early twentieth-century discussion of such terminological issues, see Alexander Lipschütz, *The internal secretions of the sex glands: The problem of the "Puberty Gland"* (Cambridge, 1924), 473.

6. Even though he concentrated exclusively on whatever was relevant to the "endocrine function of the ovary", George Corner remarked: "It would be easy enough to explain the discovery of some other endocrines, for example insulin or adrenaline, without fear of losing the thread of my discourse; but here we must follow not a single thread, but a whole tangle of them, some of which will end nowhere." See George W. Corner, "The early history of the oestrogenic hormones", *Journal of endocrinology*, xxxi (1964–65), pp. iii–xvii, p. iv.
7. For a similar argument against presentist history of medicine, see Christopher Lawrence, "'Definite and material': Coronary thrombosis and cardiologists in the 1920s", in Charles E. Rosenberg and Janet Golden (eds), *Framing disease: Studies in cultural history* (New Brunswick, 1992), 50–82, pp. 52–53. The endocrinological version of the tale I try to tell here can be found in V. C. Medvei, *The history of clinical endocrinology* (Carnforth, Lancs, 1993) and Humphry Davy Rolleston, *The endocrine organs in health and disease, with a historical review* (London, 1936). A more culturally nuanced historical approach to sex-gland research was called for more than two decades ago by Diana Long Hall, "Biology, sex hormones and sexism in the 1920s", *Philosophical forum*, v, nos 1–2 (1973/74), 81–96, a pioneering article that continues to inspire.
8. The history of the elucidation of menstruation awaits a comprehensive study. See, however, George W. Corner, "Our knowledge of the menstrual cycle, 1910–1950", *Lancet*, 28 April 1951, 919–23; and Michael J. O'Dowd and Elliot E. Philip, *The history of obstetrics and gynaecology* (London, 1994), 260–1.
9. These and many related topics are being explored in Helen Blackman's doctoral thesis in progress at the University of Manchester, "Women, savages and other animals: The comparative physiology of reproduction, c. 1850–1914".
10. Much, for instance, has been written by Anglophone scholars on the removal of ovaries in hysteria and 'nervous' disorders — but only with respect to American or British practitioners of that operation. The German gynaecologist Alfred Hegar's energetic advocacy of that operation was of great influence in his time not only in Central Europe but also in English-speaking nations. Virtually nothing, however, can be found on Hegar's work in Anglophone scholarship, a few simplistic passages in Thomas Laqueur's monograph, *Making sex: Body and gender from the Greeks to Freud* (Cambridge, Mass., 1991) representing the only exception.
11. See R. Lawson Tait, *The pathology and treatment of diseases of the ovaries (being the Hastings essay for 1873)*, 4th edn (Birmingham, 1883), 3.
12. Nelly Oudshoorn, *Beyond the natural body: An archeology of the sex hormones* (London, 1994), 19.
13. See Rudolf Virchow, "Der puerperale Zustand: Das Weib und die Zelle", in Virchow, *Gesammelte Abhandlungen zur wissenschaftlichen Medizin* (Frankfurt am Main, 1856), 735–79. On the contexts of the lecture, see Hans H. Simmer, "Zum Frauenbild Rudolf Virchows in den späten 1840er Jahren", *Medizinhistorisches Journal*, xxvii (1992), 292–319; and Simmer, "Der junge Rudolf Virchow und die Gesellschaft für Geburtshülfe in Berlin in den Jahren 1846–1848", *Sudhoffs Archiv: Zeitschrift für Wissenschaftsgeschichte*, lxxvii (1993), 72–96. All translations in this essay, unless otherwise attributed, are my own.
14. For Virchow's explanation of his unusual definition of the puerperium, see "Der puerperale Zustand" (ref. 13), 735–36.
15. On this issue, see Stefanie Holle, "Die Widerlegung des Postulates von der Gleichzeitigkeit der Ovulation und Menstruation bei der Frau: Klinische und histologische Untersuchungen

im frühe 20. Jahrhundert" (Inaugural Dissertation, Medical Faculty, University of Erlangen-Nürnberg, 1984).

16. Unfortunately, there is no comprehensive historical analysis of these important concepts and their contexts and consequences. But see Russell C. Maulitz, "The pathological tradition", in W. F. Bynum and Roy Porter (eds), *Companion encyclopaedia of the history of medicine* (London, 1993), i, 169–89, pp. 181–4; and Robert J. Miciotto, "Carl Rokitansky: A reassessment of the hematohumoral theory of disease", *Bulletin of the history of medicine*, lii (1978), 183–99.
17. See Maulitz, "Pathological tradition" (ref. 16).
18. Virchow, "Der puerperale Zustand" (ref. 13), 739–40, 746.
19. *Ibid.*, 747.
20. This shift has not been sufficiently explored by historians. But see Anne E. Walker, *The menstrual cycle* (London, 1997), 34–35.
21. On Van Helmont, see Simmer, "Zum Frauenbild Rudolf Virchows" (ref. 13), 308–10.
22. In Central Europe, Theodor Ludwig Bischoff had expressed himself almost identically in 1844. See *ibid.*, 312.
23. On the cultural importance of sensibility for the nineteenth-century German *Bildungsbürgertum*, see Joachim Radkau, *Das Zeitalter der Nervosität: Deutschland zwischen Bismarck und Hitler* (Munich, 2000), 44–45. *Cf.* the earlier (British) history of the concept in G. J. Barker-Benfield, *The culture of sensibility: Sex and society in eighteenth-century Britain* (Chicago, 1992).
24. See Carroll Smith-Rosenberg and Charles Rosenberg, "The female animal: Medical and biological views of woman and her role in nineteenth-century America", in Judith Walzer Leavitt (ed.), *Women and health in America: Historical readings* (Madison, Wisc., 1984), 12–27, p. 13.
25. The American surgeon Robert Battey initially described the removal of such ovaries as "normal ovariotomy". He soon regretted that nomenclature and clarified that "the ovaries removed, and the tubes as well, have presented visible signs of disease — signs which are evident to the naked eye and palpable to the sense of touch. For the misconception upon this point still existing, my own ignorance of both the histology and pathology of the ovaries is largely responsible in that, during the early history of the operation, I removed ovaries which I erroneously supposed to be healthy, and gave to the operation the unfortunate and now obsolete name of 'normal ovariotomy'". He listed the pathological features he began to notice once he had acquired greater knowledge of ovarian histology: small cysts, sclerosis of the investing tunic, fibrous degeneration of the stroma, salpingitis, hydrosalpinx, and pyosalpinx. See Battey's untitled contribution to T. Spencer Wells, Alfred Hegar and Robert Battey, "Castration in mental and nervous diseases: A symposium", *American journal of the medical sciences*, n.s., xcii (1886), 455–90, pp. 483–90, quoted material on pp. 483–4.
26. Although nineteenth-century medical usage was not always consistent, in English, 'oophorectomy' usually referred to the removal of ostensibly healthy or normal-sized ovaries whereas the etymologically inaccurate term 'ovariotomy' was generally reserved for the removal of cystic or otherwise enlarged ovaries. German practitioners almost invariably used the term 'c(k)astration' for bilateral oophorectomy. For the sake of clarity, I have followed the English convention throughout, even when referring to German research. For more on terminological issues, see Ornella Moscucci, *The science of woman: Gynaecology and gender in England, 1800–1929* (Cambridge, 1990), 157, 239 (n. 1); and H. H. Simmer, "Oophorectomy for breast cancer patients: Its proposal, first performance, and first explanation as an endocrine ablation", *Clio medica*, iv (1969), 227–49, p. 240, n. 1. On the history of oophorectomy in general, see Moscucci, *The science of woman*, 134–64, especially pp. 157–60; Ben Barker-Benfield, "The spermatic economy: A nineteenth century view of sexuality", *Feminist studies*, i (1972), 45–74,

pp. 60–66; Lawrence D. Longo, "The rise and fall of Battey's operation: A fashion in surgery", in Judith W. Leavitt (ed.), *Women and health in America* (ref. 24), 270–84; Laqueur, *Making sex* (ref. 10), 176–81; Andrew Scull and Diane Favreau, "'A chance to cut is a chance to cure': Sexual surgery for psychosis in three nineteenth century societies", *Research in law, deviance and social control*, viii (1986), 3–39, pp. 14–26; H. H. Simmer, "Bilaterale Oophorektomie der Frau im späten 19. Jahrhundert: Zum methodologischen Wert der Kastration für die Entdeckung ovarieller Hormone", *Geburtshilfe und Frauenheilkunde*, xliii (1983), Sonderheft, 54–59; Edward Shorter, "Medizinische Theorien spezifisch weiblicher Nervenkrankheiten im Wandel", in Alfons Labisch and Reinhard Spree (eds), *Medizinische Deutungsmacht im sozialen Wandel des 19. und frühen 20. Jahrhunderts* (Bonn, 1989), 171–80, pp. 173–6; Thilo G. Funk, "Uterine Fibromyome und Blutungen als Indikation für eine bilaterale Oophorektomie im späten 19. Jahrhundert", *Medizinhistorisches Journal*, xxi (1986), 159–71; and Günter Burger, "Nerven- und Geisteskrankheiten als Indikationen für eine bilaterale Oophorektomie im späten 19. Jahrhundert" (Inaugural-Dissertation, University of Erlangen-Nürnberg, Medical Faculty, 1984). On the use of the operation for mental and 'functional' nervous disorders in the German-speaking lands between 1872 and 1892, see Burger, "Nerven- und Geisteskrankheiten als Indikationen" (ref. 26), 24–41, 61–108, and 126–43.

27. Since the history of the introduction and dissemination of oophorectomy in Britain and America is treated in great detail by the general works cited previously, I shall concentrate on the German oophorectomy saga.
28. Many did not even agree that the ovaries were at all involved in the process — Robert Battey was, in fact, criticized for his uncritical assumption of this hypothesis. See Longo, "Rise and fall" (ref. 26), 275.
29. For a concise overview of these well-known themes, see Naomi Pfeffer, *The stork and the syringe: A political history of reproductive medicine* (Cambridge, 1993), 34–35.
30. See Longo, "Rise and fall" (ref. 26), 274.
31. Quoted in Scull and Favreau, "'A chance to cut'" (ref. 26), 15.
32. See William Goodell, *Lessons in gynecology* (Philadelphia, 1890), 394–5; and Regina Morantz and Sue Zschoche, "Professionalism, feminism, and gender roles: A comparative study of nineteenth century medical therapeutics", *Journal of American history*, xlvii (1980), 568–88.
33. See Hegar's untitled contribution (transl. by Montagu Handfield-Jones) to Spencer Wells, Hegar and Battey, "Castration in mental and nervous diseases" (ref. 25), 471–83, p. 480.
34. A. Hegar and R. Kaltenbach, *A hand-book of general and operative gynaecology*, transl. anon., vols vi–vii of Egbert H Grandin (ed.), *Cyclopaedia of obstetrics and gynaecology* (Edinburgh, 1889), vi, 303.
35. See Funk, "Uterine Fibromyome und Blutungen als Indikation für eine bilaterale Oophorektomie" (ref. 26) for a convincing demonstration of the importance of bleeding fibromyomas and the little that could be done for them before the introduction of oophorectomy — or, for that matter, blood transfusion. As another champion of oophorectomy pointed out once, there was a social component to the popularity of oophorectomy in such cases: the operation was especially suitable, he argued, for those patients whose social status did not permit them the prolonged bed-rest and comprehensive care necessary for the conservative management of profuse uterine bleeding. See H. Menzel, "Beiträge zur Castration der Frauen, I: Castrationen bei Ovarialprolaps, Uterusfibrom, Retroflexio uteri mit Descensus ovariorum und Hysterie", *AfG*, xxvi (1885), 36–57, p. 47.
36. See the overview of cases oophorectomized in Hegar's Freiburg clinic for nervous symptoms: G Schmalfuss, "Zur Castration bei Neurosen", *AfG*, xxvi (1885), 1–35.
37. See, for instance, Hermann Fehling, "Zehn Castrationen: Ein Beitrag zur Frage nach dem Werthe der Castration", *AfG*, xxii (1883), 441–55, p. 455.

38. See Menzel, "Beiträge zur Castration der Frauen, I" (ref. 35), 50–51.
39. See, for instance, Paul Flechsig, "Zur gynaekologischen Behandlung der Hysterie", *Neurologisches Centralblatt*, iii (1884), 433–9, 457–68, p. 458. Flechsig, incidentally, was a leading psychiatrist and neuroanatomist and has attained a dubious immortality as the persecutor of Daniel Paul Schreber, one of whose beliefs was that Flechsig was out to "unman" him. Morton Schatzman (*Soul murder: Persecution in the family* (Harmondsworth, 1973), 106–7) argues with reference to Flechsig's paper (above) that Schreber's doctor was undoubtedly capable of that. Schatzman does not, however, note that Flechsig's castration was of a female hysteric. For a comprehensive study of the Schreber case, see Zvi Lothane, *In defense of Schreber: Soul murder and psychiatry* (New York, 1992).
40. See Laqueur, *Making sex* (ref. 10), 177.
41. On this point, see Burger, "Nerven- und Geisteskrankheiten" (ref. 26) and W. F. Bynum, "The nervous patient in eighteenth- and nineteenth-century Britain: The psychiatric origins of British neurology", in W. F. Bynum, Roy Porter and Michael Shepherd (eds), *The anatomy of madness: Essays in the history of psychiatry* (London, 1985–88), i, 89–102.
42. See Hegar's contribution to the symposium on "Castration in mental and nervous diseases" (ref. 33), 477.
43. Hegar and Kaltenbach, *Hand-book of general and operative gynaecology* (ref. 34), vi, 307.
44. See Benedikt Stilling, *Physiologisch-pathologische und medicinisch-praktische Untersuchung über Spinal-Irritation* (Leipzig, 1840).
45. The concept of reflex neuroses was introduced by Moritz Romberg in his influential text, *Lehrbuch der Nervenkrankheiten des Menschen*, 2nd edn (Berlin, 1851), i, pt 2, 198–223, pp. 210–11. On Hegar's use of the concept (which was consistent in spite of his use of different terms to denote it at different points in his career), see Burger, "Nerven- und Geisteskrankheiten als Indikationen" (ref. 26), 7–9, 154–5. Generally, on ovarian theories of hysteria, see Ilza Veith, *Hysteria: The history of a disease* (Chicago, 1965), 173, 210, 232; and Mark Micale, "Hysteria male/hysteria female: Reflections on comparative gender construction in nineteenth-century France and Britain", in Marina Benjamin (ed.), *Science and sensibility: Gender and scientific enquiry, 1780–1945* (Oxford, 1991), 200–39, pp. 225–6.
46. The ovaries still being inaccessible except to surgeons, the uterus was often targeted for treatment in the belief that since the female nervous system was linked intimately with the reproductive system, any intervention in the latter would affect the former. This trend seems to have been particularly strong in Britain, where, in the absence of a strong tradition of 'office psychiatry', relatively minor cases of mental or nervous disorders in women were often treated by gynaecologists. See Andrew Scull, *The most solitary of afflictions: Madness and society in Britain, 1700–1900* (New Haven, Conn., 1993), 255–9; Elaine Showalter, *The female malady: Women, madness, and English culture, 1830–1980* (London, 1987), 55–57; and Charlotte Mackenzie, *Psychiatry for the rich: A history of Ticehurst Private Asylum, 1792–1917* (London, 1992), 155–6. The comparative history of the treatment of women's nervous disorders in the various European nations has yet to be written.
47. [Joseph] Amann, *Ueber den Einfluss der weiblichen Geschlechtskrankheiten auf das Nervensystem mit besonderer Berücksichtigung des Wesens und der Erscheinungen der Hysterie*, 2nd edn (Erlangen, 1874), p. IV.
48. *Ibid.*, 73–74.
49. *Ibid.*, 79.
50. *Ibid.*, 86. Not every doctor, however, had lost faith in Romberg's notion. Heinrich Kisch, Privatdozent at the German University in Prague and physician at the Marienbad spa, reiterated that hysteria was a reflex neurosis common during the two great periods of reproductive transition, puberty and menopause. Women who suffered from hysterical symptoms around

menopause had almost invariably suffered from similar symptoms around puberty. See E. Heinrich Kisch, *Das klimakterische Alter der Frauen in physiologischer und pathologischer Beziehung* (Erlangen, 1874), 69, 138.

51. See F. Jolly, "Hysterie und Hypochondrie", in A. Eulenburg, H. Nothnagel, J. Bauer, H. von Ziemssen and F. Jolly, *Handbuch der Krankheiten des Nervensystems II*, = xii/2 (1877) of H. von Ziemssen (ed.), *Handbuch der speciellen Pathologie und Therapie*, 2nd edn (17 vols in numerous parts, Leipzig, 1876–85), 489–709, pp. 495, 501–2.
52. *Ibid.*, 509.
53. A. Rheinstaedter, "Ueber weibliche Nervosität, ihre Beziehungen zu den Krankheiten der Generationsorgane und ihre Allgemeinbehandlung", *Sammlung klinischer Vorträge*, clxxxviii, Gynäkologie no. lvi (1880), 1493–510, p. 1495.
54. A. Döderlein and B. Krönig, *Operative Gynäkologie* (Leipzig, 1905), 594.
55. See *ibid.*, 595–6 and O. Binswanger, *Die Hysterie* (1904), = xii/1, section 2 of Hermann Nothnagel *et al.* (eds), *Specielle Pathologie und Therapie* (24 vols in numerous parts, Vienna, 1894–1908), 59–60, 843–4, 945–6 and, on the degenerative basis of hysteria, 36–44.
56. Döderlein and Krönig, *op. cit.* (ref. 54), 596–7.
57. A. Hegar, "Die Castration der Frauen", in Richard Volkmann (ed.), *Sammlung klinischer Vorträge*, nos cxxxvi–cxxxviii, Gynäkologie no. xlii (1878), 925–1068, p. 1001.
58. Hegar and Kaltenbach, *A hand-book of general and operative gynaecology* (ref. 34), vi, 300. For a powerful challenge to Hegar's argument that oophorectomy did not reduce sex drive, see Ludwig Glaevecke, "Körperliche und geistige Veränderungen im weiblichen Körper nach künstlichem Verluste der Ovarien einerseits und des Uterus andererseits", *AfG*, xxxv (1889), 1–88, pp. 53–55.
59. F. Keppler, "Das Geschlechtsleben des Weibes nach der Kastration", *Wiener medizinische Wochenschrift*, xxxxi (1891), 1489–92 and 1523–6, p. 1525.
60. See Hegar's contribution to "Castration in mental and nervous Diseases: A symposium" (ref. 33), 471.
61. Hegar, "Die Castration der Frauen" (ref. 57), 926–31.
62. Even the well-known physical changes after menopause he was inclined to attribute at least partly to ageing in general and the mental changes to experience. See *ibid.*, 1002.
63. *Ibid.*, 1000.
64. See A. Hegar, "Ueber die Exstirpation normaler und nicht zu umfänglichen Tumoren degenerirter Eierstöcke", *CfG*, i (1877), 297–306, pp. 298–9.
65. *Ibid.*, 305; and "Die Castration der Frauen" (ref. 57), 1004–5.
66. See Glaevecke, *op. cit.* (ref. 58), 31–32, 50.
67. See Jutta Blönnigen, "Die Osteomalazie als Indikation für eine bilaterale Oophorektomie im späten 19. u. frühen 20. Jahrhundert: Ergebnisse und Erklärungsversuche" (Inaugural-Dissertation, Medical Faculty, University of Erlangen-Nürnberg, 1980); and Simmer, "Oophorectomy for breast cancer patients" (ref. 26).
68. It is the adult equivalent of rickets and shares the same causal factors.
69. The fewer cases of osteomalacia in men were often considered to be fundamentally different. See H. Fehling, "Ueber Wesen und Behandlung der puerperalen Osteomalakie", *AfG*, xxxix (1891), 171–96, pp. 180–1.
70. On contemporary convictions of endemicity, see, for instance, W. Thorn, "Zur Kasuistik der Kastration bei Osteomalakie", *CfG*, xv (1891), 828–31.
71. See Adolf Strümpell, *Lehrbuch der speciellen Pathologie und Therapie der inneren Krankheiten* (Leipzig, Vogel, 1883–84), ii, pt 2, 169–72.
72. See Paul Zweifel, "Zur Discussion über Porro's Methode des Kaiserschnittes", *AfG*, xvii (1881),

355–77; *idem*, "Ein Fall von Osteomalacie, modificirter Porro-Kaiserschnitt, geheilt", *CfG*, xiv (1890), 25–29, esp. p. 28.

73. Blönnigen, "Die Osteomalazie als Indikation für eine bilaterale Oophorektomie" (ref. 67), 10–12. Fehling rejected suggestions that the prevention of pregnancy by tubal ligation would be sufficient to relieve osteomalacia, pointing out that of his eight cases till date, the majority (six) had had their last pregnancies more than two years ago. See H. Fehling, "Zur Frage der Therapie bei Osteomalacie", *CfG*, xiv (1890), 73–74.
74. See P. Zweifel, "Hermann Johannes Karl Fehling", *CfG*, xlix (1925), 2866–74; and K. Franz, "Hermann Fehling", *AfG*, cxxvii (1926), pp. I–IV.
75. H. Fehling, "Über Kastration bei Osteomalacie", *Verhandlungen der Deutschen Gesellschaft für Gynäkologie*, ii (1888), 311–18. Fehling also reported on two other operations in this paper. In one, the operation had led to immediate improvement but the patient had soon relapsed and the other patient, although apaprently recovering, had been operated upon too recently to be commented upon.
76. See Blönnigen, *op. cit.* (ref. 67), 29–39.
77. A proportion did show some relatively minor (and quite inconsistent) abnormalities such as hypertrophy, atrophy, cysts, hyaline degeneration and chronic oophoritis. See Blönnigen, *op. cit.* (ref. 67), 31–39.
78. See Blönnigen, *op. cit.* (ref. 67), 51.
79. H. Fehling, "Ueber Wesen und Behandlung der puerperalen Osteomalakie", *AfG*, xxxix (1891), 171–96; and *idem*, "Weitere Beiträge zur Lehre von der Osteomalakie", *AfG*, xxxxviii (1895), 472–98.
80. On support for the neural hypothesis, see M. Hofmeier, *CfG*, xv (1891), 225–8; Thorn, *op. cit.* (ref. 70); H. Eisenhart, "Beiträge zur Aetiologie der puerperalen Osteomalacie", *Deutsches Archiv für klinische Medizin*, xlix (1892), 156–205; and Guillaume Rossier, "Anatomische Untersuchung der Ovarien in Fällen von Osteomalacie", *AfG*, xlviii (1895), 472–98.
81. F. von Winckel, "Über die Erfolge der Kastration bei Osteomalakie", *Sammlung klinischer Vorträge*, n.s., lxxi, Gynäkologie no. xxviii (1893), 657–82, pp. 673–4.
82. See Blönnigen, *op. cit.* (ref. 67), 60–61. Fehling ('Weitere Beiträge zur Lehre von der Osteomalakie" (ref. 69), 484–5) pointed out that patients of osteomalacia had never improved after being chloroformed for purposes other than oophorectomy or after taking chloral as a sleeping draught. Oophorectomy even under anaesthesia with ether, on the other hand, had led to striking results.
83. Hermann Senator, "Zur Kenntniss der Osteomalacie und der Organotherapie", *Berliner klinische Wochenschrift*, xxxiv (1897), 109–12, 143–4; and Wilhelm Latzko and Julius Schnitzler, "Ein Beitrag zur Organotherapie bei Osteomalacie", *Deutsche medizinische Wochenschrift*, xxiii (1897), 587–92.
84. See, for example, Ernst Hoennicke, "Zur Theorie der Osteomalacie: Zugleich zur Lehre von den Krankheiten der Schilddrüse", *Berliner klinische Wochenschrift*, xxxxi (1904), 1154–6; M. L. Bossi, "Die Nebennieren und die Osteomalacie", *AfG*, lxxxiii (1907), 505–44; Hans Bab, "Die Behandlung der Osteomalacie mit Hypophysenextrakt", *Münchener medizinische Wochenschrift*, lxv (1911), 1814–17; Hans Curschmann, "Über den mono- und pluriglandulären Symptomenkomplex der nichtpuerperalen Osteomalacie", *Deutsches Archiv für klinische Medizin*, cxxix (1919), 93–117; and Naegeli, "Uebersicht über die Symptomatik der Osteomalazie als innersekretorischer pluriglandulärer Erkrankung", *Münchener medizinische Wochenschrift*, lxv (1918), 585–6.
85. See, for instance, Döderlein and Krönig, *Operative Gynäkologie* (ref. 54), 540–2.
86. M. Hofmeier, "Zur Frage der Behandlung der Osteomalacie durch Kastration", *CfG*, xv (1891),

225–8, pp. 224, 227. Why removing the ovaries was effective in osteomalacia, however, is a question that cannot, as Jutta Blönnigen emphasizes, be solved even with the aid of today's knowledge of endocrinology, and no consensus was ever reached on the issue in the earlier decades of the century. See Blönnigen, *op. cit.* (ref. 67), 84.

87. See G. T. Beatson, "On the treatment of inoperable cases of carcinoma of the mamma: Suggestions for a new method of treatment with illustrative cases", *The lancet*, 11 July 1896, 104–7, and 18 July 1896, 162–5. On his life and career, see the obituary by G. H. Edington in the *British medical journal*, 25 February 1933, 344–5. Beatson's experiments are sometimes presented quite unjustifiably by today's clinicians as pioneering examples of "hormonal cancer therapeutics". See, for example, Tim Gulliford and Richard J. Epstein, "Endocrine treatment of cancer", *Journal of the Royal Society of Medicine*, lxxxix (1996), 448–53, quoted phrase on p. 448. A somewhat more nuanced presentation is Robin Leake, "100 years of the endocrine battle against breast cancer", *Lancet*, cccxlvii (1996), 1780–1.
88. Beatson, "On the treatment of inoperable cases of carcinoma" (ref. 87), 106.
89. *Ibid*., 106.
90. *Ibid.*, 106.
91. *Ibid*., 107. In a later article, he stated that thyroid "powerfully affects the metabolism generally of the body cells, raising their tone and improving their vigour, while it acts favourably on the lymphatic system, lessening the chances of dissemination by it". See G. T. Beatson, "The treatment of cancer of the breast by oöphorectomy and thyroid extract", *British medical journal*, 19 October 1901, 1145–8, p. 1147.
92. Beatson, "On the treatment of inoperable cases of carcinoma" (ref. 87), 163.
93. Beatson, "The treatment of cancer of the breast by oöphorectomy and thyroid extract" (ref. 91), 1146.
94. Beatson, "On the treatment of inoperable cases of carcinoma" (ref. 87), 163.
95. *Ibid.*, 164. Beatson thought that the testicles probably had a similar influence on cancer in men: "I am making inquires as to the existence of cancer amongst eunuchs, for if my view is correct they should not suffer from it" (*ibid.*). In the late nineteenth century, many theories of cancer argued that it was a parasitic disease. On these, see Jacob Wolff, *The science of cancerous disease from earliest times to the present*, transl. by Barbara Ayoub (Canton, Mass., 1989), 431–590.
96. See Simmer, "Oophorectomy for breast cancer patients" (ref. 26), 232–3.
97. G. T. Beatson, "Remarks on the etiology of carcinoma: Has it a physiological function in the body?", *British medical journal*, 29 April 1905, 921–5, p. 924.
98. Stanley Boyd, "On oophorectomy in the treatment of cancer", *British medical journal*, 2 October 1897, 890–6. Boyd did not, however, use thyroid extract until the oophorectomy had obviously failed to bring about any improvement. He later explained that "when endeavouring to ascertain the effect of one mode of treatment based upon highly theoretical considerations it is surely unwise to combine it with another method resting on still more shadowy grounds". Even when he used thyroid in the failing cases, he noticed no beneficial effects. See S. Boyd, "On oophorectomy in cancer of the breast", *British medical journal*, 20 October 1900, 1161–7, p. 1166.
99. Boyd, "On oophorectomy in the treatment of cancer" (ref. 98), 895.
100. *Ibid.*, 896.
101. Boyd, "On oöphorectomy in cancer of the breast" (ref. 98).
102. *Ibid.*, 1166.
103. *Ibid.*, 1166.
104. Less well-known, even in its time, was the 1874 report of physiologist Friedrich Goltz (1834–1902)

that a bitch with her spinal cord transected at the level of the first cervical vertebra had gone into oestrus, mated (with a male toward whom she had previously been antagonistic), and became pregnant with triplets. It had long been known that oestrus did not occur in the absence of the sex glands but Goltz's experiment began to indicate how the sex glands might affect the brain to produce the mating urge characterizing oestrus. In an intact animal, Goltz argued, one could assume that centripetal nerve fibers from the sex glands stimulated the brain to produce oestrus but that hypothesis was obviously inapplicable to an animal whose spinal cord had been completely separated from the brain. Goltz, therefore, concluded that the sex glands exerted their effect on the brain by releasing specific chemical substances into the blood during oestrus. See F. Goltz and A. Freusberg, "Ueber den Einfluss des Nervensystems auf die Vorgänge während der Schwangerschaft und des Gebärakts", *Archiv für die gesammte Physiologie*, ix (1874), 552–65. If one refused to surrender the neural perspective, the only available option, said Goltz, was to assume that the sex glands and the brain were not connected through the spinal cord but through that portion of the autonomic nervous system, which originated from the spinal cord *above* the level of transection and did not 'travel' with it. Such a hypothesis was plausible but the chemical theory, he believed, was more reasonable. Hans Simmer has pointed out that despite its acuity, Goltz's hypothesis was virtually ignored. This, says Simmer, may have been caused by the lack of a theoretical basis, which became available only after Brown-Séquard's expansion of Claude Bernard's concept of internal secretion into a system of regulation akin to the nervous system. See Simmer, "Bilaterale Oophorektomie der Frau" (ref. 26), 57. This is certainly true for the 1870s but why was Goltz's work not resurrected and celebrated in the 1890s? Because, I would suggest, the 1890s was the heroic age of *gynaecological* research on the ovaries: the vast majority of researchers were clinical gynaecologists and the bulk of their research was conducted with explicit clinical motivations. They may not even have been aware of a physiologist's study (even though it was published in the leading physiological journal of the era) that had raised few ripples within physiological circles. The only turn-of-the-century instance of an enthusiastic recognition of the implications of Goltz's experiment that I know of was not of a scientist but that of the well-informed, idiosyncratic Viennese philosopher Otto Weininger (1880–1903). On Weininger's use of Goltz's experiment and other early reports on the origin and nature of sex, see Chandak Sengoopta, *Otto Weininger: Sex, science, and self in Imperial Vienna* (Chicago, 2000), 74–76.

105. H. M. Evans, "Present position of our knowledge of anterior pituitary function", *Journal of the American Medical Association*, ci (1933), 425–32, p. 425. Evans specifically mentioned Brown-Séquard and the controversial physiologist of the 1920s, Eugen Steinach (1861–1944). On the former, see Merriley Borell, "Origins of the hormone concept: Internal secretions and physiological research, 1889–1905", Ph.D. thesis, Yale University, 1976. On Steinach, see below.
106. See Merriley Borell, "Organotherapy and the emergence of reproductive endocrinology", *Journal of the history of biology*, xviii (1985), 1–30.
107. W. S. Bainbridge, "Transplantation of human ovaries: Present status and future possibilities", *American journal of obstetrics and gynecology*, v (1923), 493–8, pp. 493–4.
108. Bainbridge was neither the first gynaecologist nor the only one to express similar views. In 1902, for example, another American gynaecologist had written: "That the removal of healthy tissue is bad surgery has always been an axiom among operators. Many have reproached the gynecologist for being the most marked offender in this regard, and certainly it would seem that there has been some truth in the charge ... in the early days of operative gynecology, not so many years ago, the removal of diseased structures previously considered as beyond the province of the surgeon was looked upon as leaving but little to be desired. Careful study of the after histories of operative cases soon showed however, that although the patient might recover from the immediate operation, she might still be the victim of symptoms of a subjective

nature, which made her life miserable. These symptoms were soon rightly attributed to the castration, and conservative gynecology owes its existence to the attempt to escape the results of the operative menopause". See W. R. Nicholson, "A review of the literature of ovarian transplantation", *University of Pennsylvania medical bulletin*, xiv (1901–2), 401–7, p. 401. Nicholson was Instructor in Gynaecology at the University of Pennsylvania.

109. See Merriley Borell, "Brown-Sequard's organotherapy and its appearance in America at the end of the nineteenth century", *Bulletin of the history of medicine*, l (1976), 309–20; and *idem*, "Organotherapy, British physiology, and discovery of the internal secretions", *Journal of the history of biology*, ix (1976), 235–68.
110. See Corner, "Early history" (ref. 6), p. vi.
111. See H. H. Simmer, "Organotherapie mit Ovarialpräparaten in der Mitte der neunziger Jahre des 19. Jahrhunderts: Medizinische und pharmazeutische Probleme", in Erika Hickel and Gerald Schröder (eds), *Neue Beiträge zur Arzneimittelgeschichte* (Stuttgart, 1982), 229–64.
112. Corner, "Early history" (ref. 6), p. vi.
113. See Erna Lesky, *The Vienna Medical School of the 19th century*, transl. by L. Williams and I. S. Levij (Baltimore, 1976), 427–33. Quoted material on pp. 427 and 487.
114. See R. Chrobak, "Über Einverleibung von Eierstocksgewebe", *CfG*, xx (1896), 521–4.
115. See Borell, "Organotherapy, British physiology and discovery of the internal secretions" (ref. 109).
116. Chrobak, "Ueber Einverleibung von Eierstocksgewebe" (ref. 114), 522. Chrobak referred to an earlier report by F. Mainzer, "Vorschlag zur Behandlung der Ausfallserscheinungen nach Castration", *Deutsche medizinische Wochenschrift*, xxii (1896), 188. Mainzer had reported on the successful treatment of post-oophorectomy symptoms in one patient with ovarian extracts and had proffered the same analogy with thyroid extracts. The sequelae of thyroidectomy were well-documented by the 1880s, and were being treated with thyroid extracts by 1891: this has often been seen as "the first generally recognized success of organotherapy". See Medvei, *History of clinical endocrinology* (ref. 7), 160–62, quote from p. 162.
117. He reported having administered powdered ovarian substance to eight patients in all, with no controls and sometimes no follow-up. Of those, three patients reported a significant diminution of symptoms — of the others, one had dropped out and the rest had only very recently been put on the treatment. The powder had not caused any negative symptoms in any of the patients. Chrobak admitted that his uncontrolled experiments did not permit any conclusions about the efficacy of the treatment or its mechanism of action. See Chrobak, "Ueber Einverleibung von Eierstocksgewebe" (ref. 114), 523–4.
118. It is interesting to compare this with the independent introduction of oophorectomy by three surgeons in the United States, Britain, and Germany in 1872. See Simmer, "Bilaterale Oophorectomie der Frau" (ref. 26).
119. The two were Theodor Landau (1861–1932) of Berlin and Richard Mond (b. 1867) of Kiel. Landau's idea was tested in the gynaecological clinic of his elder brother Leopold Landau by the latter's assistant, Ferdinand Mainzer. Mainzer's preliminary report was published before Chrobak's and was cited by the latter. See Simmer, "Organotherapie mit Ovarialpräparaten" (ref. 111). The analogy between the thyroid and the ovary was also used, albeit less frequently, to justify the use of ovarian extracts in women suffering from hypothyroidism. See René Moreau, *De l'opothérapie ovarienne dans la maladie de Basedow chez la femme* (Paris, 1899).
120. See Hans H. Simmer, "Innere Sekretion der Ovarien als Ursache der Menstruation: Halbans Falsifikation der Pflügerschen Hypothese", in Kurt Ganzinger, Manfred Skopec and Helmut Wyklicky (eds), *Festschrift für Erna Lesky zum 70. Geburtstag* (Vienna, 1981), 123–48, p. 124.
121. Corner, "Early history" (ref. 6), p. vi.

122. Many medical scientists, beginning with Moritz Schiff in 1884, had reported that thyroid grafting was feasible and effective in removing symptoms of thyroid deficiency, especially those following thyroidectomy. See Medvei, *History of clinical endocrinology* (ref. 7), 136, 160–2.

123. Chrobak and Knauer were apparently unaware then that ovarian transplantations were being attempted on humans by the American surgeon Robert T. Morris (1857–1945), who, too, was interested in allaying the sequelae of oophorectomy and influenced by reports of successful thyroid grafting. Unlike the former, however, Morris also believed that ovarian grafts might facilitate pregnancy in women whose fallopian tubes were blocked and, therefore, unable to convey the ovum from the ovary to the uterus. If ovarian tissue could be grafted on to the uterus itself, the fallopian tubes would not be required any more. Morris reported on two patients, one of whom soon became pregnant but then had a miscarriage. By 1901, Morris had performed twelve ovarian transplants. And in 1906, one of his transplantees had a baby. For further information, see R. T. Morris, *Lectures on appendicitis and notes on other subjects* (New York, 1895), 156–9; *idem*, "The ovarian graft", *New York medical journal*, lxii (1895), 436–7; Hans H. Simmer, "Robert Tuttle Morris (1857–1945): A pioneer in ovarian transplants", *Obstetrics and gynecology*, xxxv (1970), 314–28; and J. D. Biggers, "In vitro fertilization and embryo transfer in historical perspective", in Alan Trounson and Carl Wood (eds), *In vitro fertilization and embryo transfer* (London, 1984), 3–15, pp. 5–6 (my thanks to Dr Biggers for alerting me to his article). Another early report on ovarian transplantation in humans also came from the U.S. See James Glass, "An experiment in transplantation of the entire human ovary", *Medical news*, lxxiv (1899), 523–5. Glass (1854–1931) was well acquainted with the work of Knauer but not with that of Morris! In his first report in 1896, Knauer referred to experimental transplantations of the testes but declared that he did not know of any experimental transplantations of the ovaries. See E. Knauer, "Einige Versuche über Ovarientransplantation bei Kaninchen: Vorläufige Mitteilung", *CfG*, xx (1896), 524–8, p. 528. Later, in his detailed 1900 paper, Knauer referred to the reports of Morris and Glass. See Knauer, "Die Ovarientransplantation: Experimentelle Studie", *AfG*, lx (1900), 322–75, pp. 323–4.

124. The other gynaecologists who noticed the occurrence of premature menopause after oophorectomy were equally silent on the possibility that the ovary produced an internal secretion affecting the female reproductive organs. Their theories relied mostly upon nervous or vascular links. See Simmer, "Organotherapie mit Ovarialpräparate" (ref. 111), 243–5. Simmer suggests that this might have been due to the disrepute into which Brown-Séquard's rejuvenation claims had fallen. For Knauer's dismissal of nervous and vascular theories, see "Die Ovarientransplantation: Experimentelle Studie" (ref. 123), 351–3.

125. See Knauer, "Einige Versuche über Ovarientransplantation bei Kaninchen: Vorläufige Mitteilung" (ref. 123); *idem*, "Über Ovarientransplantation," *Wiener klinische Wochenschrift*, xii (1899), 1219–22; and *idem*, "Die Ovarientransplantation: Experimentelle Studie" (ref. 123). For an attempted refutation of Knauer's claims, see E. Arendt, "Demonstration und Bemerkungen zur Ovarientransplantation", *CfG*, xxii (1898), 1116–17, and for a rebuttal, Knauer, "Zu Dr. Arendt's 'Demonstration und Bemerkungen zur Ovarientransplantation' auf der 70. Versammlung deutscher Naturforscher und Ärzte zu Düsseldorf", *ibid*., 1257–60. On Knauer, see Hermann Knaus, "Emil Knauer, Graz", *AfG*, clix (1935), 429–31.

126. See Knauer, "Über Ovarientransplantation" (ref. 125), 1220.

127. See E. Knauer, "Zur Ovarientransplantation (Geburt am normalen Ende der Schwangerschaft nach Ovarientransplantation beim Kaninchen)", *CfG*, xxii (1898), 201–3.

128. Knauer pointed out that rabbits generally experienced menopause in 5–6 years and his experimental animal was already over a year old at the time of the transplantation. See Knauer, "Die Ovarientransplantation: Experimentelle Studie" (ref. 125), 340.

129. Knauer, "Über Ovarientransplantation" (ref. 125), 1222.
130. On Halban's career and research, see Robert Köhler, "Josef Halban", *CfG*, lxi (1937), 1458–66; H. H. Simmer, "Josef Halban (1870–1937): Pionier der Endokrinologie der Fortpflanzung", *Wiener medizinische Wochenschrift*, cxxi (1971), 549–52; *idem*, "Innere Sekretion der Ovarien als Ursache der Menstruation" (ref. 120); and Norbert Pecher, "Halbans Lehre von der protektiven Wirkung der Sexualhormone: Eine frühe Konzeption über den Wirkungsmechanismus der Hormone" (Inaugural-Dissertation, Medical Faculty, University of Erlangen-Nürnberg, 1985).
131. See J. Halban, Discussion on E. Knauer, "Über Ovarientransplantation", *Wiener klinische Wochenschrift*, xii (1899), 1243–4. In his final, comprehensive report on his transplantation experiments published in 1900, Knauer referred for the first time to the internal secretions almost in identical words. See Knauer, "Die Ovarientransplantation" (ref. 123), 354.
132. See Nicholson, "A review of the literature of ovarian transplantation" (ref. 108).
133. Many of them were by biologists who seemed intent on demonstrating the mere possibility of ovarian transplantation and finding ever more sophisticated techniques for the operation. See, for example, Woldemar Grigorieff, "Die Schwangerschaft bei der Transplantation der Eierstöcke", *CfG*, xxi (1897), 663–8; Hugo Ribbert, "Über Transplantation von Ovarium, Hoden und Mamma", *Archiv für Entwickelungsmechanik der Organismen*, vii (1898), 688–708; G. L. Basso, "Über Ovarientransplantation", *AfG*, lxxvii (1906), 51–62; and Shigeji Higuchi, "Über die Transplantation der Ovarien", *ibid.*, xci (1910), 214–42. For a review of these experiments, see W. E. Castle and John C. Phillips, *On germinal transplantation in vertebrates* (Washington, DC, 1911), 2–6; and Knud Sand, "Transplantation der Keimdrüsen bei Wirbeltieren", in A. Bethe, G. von Bergmann, G. Embden and A. Ellinger (eds), *Handbuch der normalen und pathologischen Physiologie* (18 vols in 25, Berlin, 1925–32), xiv, pt 1, 251–92, pp. 274–92.
134. There was also a strong body of opinion which held that the so-called "ablation symptoms are due to a breaking of the utero-ovarian harmony, and that if the uterus is removed the retention of ovarian tissue in situ or by transplantation is of little physiological value". See Franklin H. Martin, "Ovarian transplantation: I. Brief abstract of articles published in 1917 to 1921 inclusive. II. Summary of abstracts arranged by subjects. III. Author's conclusions. IV. Exhaustive bibliography", *Surgery, gynecology and obstetrics*, xxxv (1922), 573–85, pp. 573, 582.
135. See, for instance, the negative opinion of F. Unterberger, "Hat die Ovarientransplantation praktische Bedeutung?", *Deutsche medizinische Wochenschrift*, xliv (1918), 903–4. Many researchers accepted that tissue antagonisms between donor and recipient were the likely cause of graft failures. See Franklin H. Martin, "Ovarian transplantation: A review of the literature and bibliography up to and including the earlier months of 1915", *Surgery, gynecology and obstetrics*, xxi (1915), 568–78, p. 572.
136. M. Nussbaum, "Innere Sekretion und Nerveneinfluss", *Ergebnisse der Anatomie und Entwickwlungsgeschichte*, xv (1905), 39–89, pp. 78–80.
137. Lipschütz, *Internal secretions of the sex glands* (ref. 5), 92.
138. Corner, "Early history" (ref. 6), p. xi. Corner added that in 1924, there were powerful voices — neither uninformed nor unsympathetic to endocrine physiology — that were even more sceptical than he was himself. See A. J. Carlson, "Physiology of the mammalian ovaries", *Journal of the American Medical Association*, lxxxiii (1924), 1920–3.
139. See obituary in *Nature*, cxiv (1924), 904.
140. Hans H. Simmer, "The first experiments to demonstrate an endocrine function of the corpus luteum: on the occasion of the 100. birthday of Ludwig Fraenkel (1870–1951)", *Sudhoffs Archiv*, lv (1971), 392–417.
141. A. Prenant, "La valeur morphologique du corps jaune, son action physiologique et thérapeutique

possible", *Revue générale des sciences pures et appliquées*, ix (1898), 646–50.

142. See H. H. Simmer, "On the history of hormonal contraception, I: Ludwig Haberlandt (1885–1932) and his concept of 'hormonal sterilization'", *Contraception*, i (1970), 3–27, pp. 5–9.
143. See Simmer, "The first experiments to demonstrate an endocrine function of the corpus luteum" (ref. 140). Fraenkel had been trained in gynaecology by, among others, Alfred Hegar (*ibid.*, 403). Born's idea was also tested by another of his students, the Norwegian neurosurgeon Vilhelm Magnus (see Hans H. Simmer, "The first experiments to demonstrate an endocrine function of the corpus luteum, Part II: Ludwig Fraenkel versus Vilhelm Magnus", *Sudhoffs Archiv*, lvi (1972), 76–99).
144. Quoted by Simmer, "The first experiments" (ref. 140), 399.
145. For a comprehensive report of Fraenkel's experiments and his suggestions on clinical applications, see L. Fraenkel, "Die Funktion des Corpus luteum", *AfG*, lxviii (1903), 438–545. For a briefer presentation of his conclusions and comments by other scientists of the time, see L. Fraenkel, "Weitere Mitteilungen über die Funktion des Corpus luteum", *CfG*, xxviiii (1904), 621–36, 657–68.
146. Fraenkel, "Weitere Mitteilungen" (ref. 145), 624.
147. It was Vilhelm Magnus's less acclaimed research that indicated that other portions of the ovary might produce their own, different internal secretions. See Simmer, "The first experiments to demonstrate an endocrine function of the corpus luteum, Part II" (ref. 143), 80.
148. See Fraenkel, "Die Funktion des Corpus luteum" (ref. 145), 439.
149. For Halban's comments, see Fraenkel, "Weitere Mitteilungen" (ref. 145), 628–32, and for Schauta's, *ibid.*, 660–1.
150. Simmer, "The first experiments to demonstrate an endocrine function of the corpus luteum, Part II" (ref. 143), 82.
151. Fraenkel, "Die Function des Corpus luteum" (ref. 145), 439.
152. *Ibid.*, 489.
153. *Ibid.*, 489–91.
154. *Ibid.*, 496.
155. *Ibid.*, 492. Fraenkel provided the address of a chemist from whom Lutein could be obtained at the rate of 4.50 Marks for 100 tablets. See *ibid.*, fn (1). Fifteen brief case histories were included in the paper: see *ibid.*, 492–5.
156. Fraenkel, "Die Function des Corpus luteum" (ref. 145), 496.
157. *Ibid.*, 497; "Weitere Mitteilungen" (ref. 145), 622.
158. Fraenkel, "Die Function des Corpus luteum" (ref. 145), 498–9.
159. See Fraenkel, "Weitere Mitteilungen" (ref. 145), 621–2.
160. *Ibid.*, 627–8. The Beard-Prenant hypothesis, of course, had claimed that it was the corpus luteum that prevented ovulation and the experimental work of Leo Loeb had shown in 1909 that the corpus luteum was one of the important agencies for preventing ovulation (see L. Loeb, "The experimental production of the maternal placenta and the function of the corpus luteum", *Journal of the American Medical Association*, 30 October 1909, 1471–4, p. 1472), which was confirmed by other researchers. Hence, it was actually more logical to use the corpus luteum (or, indeed, the ovary as a whole) to prevent pregnancy and this was attempted successfully in the 1920s by the Innsbruck physiologist Ludwig Haberlandt, who assumed that the corpus luteum and other internal secretory cells of the ovary might inhibit ovulation. Haberlandt transplanted ovaries of pregnant rabbits in non-pregnant animals which resulted frequently in temporary sterilization. For further information, see H. H. Simmer, "On the history of hormonal contraception, I" (ref. 142), and *idem*, "On the history of hormonal contraception, II: Ottfried Otto Fellner (1873–19??) and estrogens as antifertility hormones", *Contraception*,

iii, no. 1 (1971), 1–20.

161. Artur Biedl, *The internal secretory organs: Their physiology and pathology*, transl. by Linda Forster (London, 1913), 398–410. This was a translation of the first edition of Biedl's *Innere Sekretion: Ihre physiologischen Grundlagen und ihre Bedeutung für die Pathologie* (Berlin, 1910).

162. V. B. Green-Armytage, "The rise of surgical gynaecology", in J. M. Munro-Kerr, R. W. Johnstone and Miles H. Phillips (eds), *Historical review of British obstetrics and gynaecology 1800–1950* (Edinburgh, 1954), 357–69, p. 363.

163. For biographical accounts, see John Peel, *William Blair-Bell: Father and founder* (London, 1986); and D'Arcy Power and W. R. Le Fanu, *Lives of the Fellows of the Royal College of Surgeons of England 1930–1951* (London, 1953), 85–87.

164. Bell saw gynaecology as "a science limited no longer ... to the 'region below the belt', but embracing many aspects of medicine and surgery, to both of which, indeed, it has itself largely contributed. Nevertheless, physicians and surgeons in the past ... have treated gynaecology and obstetrics rather disdainfully as an offshoot, and not as a partner of equal rank. Our art, which is as old as theirs, and which has also attained to the position of a high biological science ... has its own individuality and ideals, which must find expression at the hands of its own exponents". W. Blair-Bell, Letter to *British medical journal*, 23 March 1929, 572, quoted in William Fletcher Shaw, *Twenty-five years: The story of the Royal College of Obstetricians and Gynaecologists, 1929–1954* (London, 1954), 31–33.

165. In a new, historical introduction written for the last edition of his textbook of gynaecology, Bell observed: "Many of us look forward to that day when knowledge of the whole of the Genital System in Woman — its morphology, physiology and pathology — will be included in the word 'gynaecology', and when 'obstetrics' will form a natural subdivision only of the inclusive subject". See William Blair-Bell, *The principles of gynaecology: A text-book for students and practitioners*, 4th edn, rev. with the assistance of M. M. Datnow and Arthur C. H. Bell (London, 1934), 1.

166. *Ibid.*

167. W. Blair-Bell, "The Arris and Gale Lectures on the genital functions of the ductless glands in the female", *Lancet*, i (1913), 809–16, 937–44, p. 809, emphasis added.

168. W. Blair-Bell, *The sex complex: A study of the relationships of the internal secretions to the female characteristics and functions in health and disease* (London, 1916), 22.

169. The generalist–specialist debate was fundamental to nineteenth-century British medicine and although no longer as heated in Bell's time as it had been some decades earlier, many general physicians and surgeons still considered specialists (of whom gynaecologists were a particularly controversial group) to be ignorant of all bodily parts or functions save the one they claimed to know. See M. Jeanne Peterson, *The medical profession in mid-Victorian London* (Berkeley, 1978), 273–5, 277–9; William F. Bynum, *Science and the practice of medicine in the nineteenth century* (Cambridge, 1994), 191–6; and Moscucci, *The science of woman* (ref. 26), 57–59, 72–81.

170. In 1891, the eminent British surgeon Thomas Spencer Wells defined the ovary as "the nucleus of gynaecological science and the source of gynaecological practice". See Moscucci, *The science of woman* (ref. 26), 158. For some discussion of the influence of other ductless glands on sex, see Francis H. A. Marshall, *The physiology of reproduction*, 2nd edn (London, 1922), 379–86.

171. Blair-Bell, "Arris and Gale Lectures" (ref. 167), 809.

172. Blair-Bell, *Sex complex* (ref. 168), p. vii (emphasis in the original), 5.

173. *Ibid.*, 23. With regard to the corpus luteum, he remarked: "Although it is well known that the corpus luteum in the ovary of the pregnant female is considerably larger than in the

non-pregnant, it is not certain that this hyperplasia has any more importance than an epiphenomenon" (*ibid.*).

174. Blair-Bell, *Sex complex* (ref. 168), 30–35, 46–47.
175. *Ibid.*, 35. "Any influence", Bell added, "the ovary may have over the general metabolism is ... related to and dependent on its primary reproductive functions. I do not believe that this organ influences the metabolism except in so far as this special function is concerned". See *ibid.*, 97.
176. The thyroid vesicles were distended with colloid after oophorectomy and the colloid was no longer acidophilic as it was in normal conditions. It was likely, however, that this colloid was a storage secretion of no great physiological significance. See *ibid.*, 37.
177. *Ibid.*, 39–41.
178. *Ibid.*, 48–55.
179. See W. Blair-Bell, "The pituitary body and the therapeutic value of the infundibular extract in shock, uterine atony, and intestinal paresis", *British medical journal*, ii (1909), 1609–13; and *idem*, *The pituitary: A study of the morphology, physiology, pathology, and surgical treatment of the pituitary, together with an account of the therapeutical uses of the extracts made from this organ* (London, 1919).
180. W. Blair-Bell and Pantland Hick, "Observations on the physiology of the female genital organs", *British medical journal*, i (1909), 517–22, 592–7, 655–8, 716–18, 777–83. The uterine effect of posterior pituitary extract had previously been noted by Henry H. Dale, who had supplied Bell with the extract (*ibid.*, 779).
181. It was realized only subsequently that the extract was separated into pressor and oxytocic fractions. See J. M. Munro Kerr, "Labour", in Munro Kerr, Johnstone and Phillips (eds), *Historical review of British obstetrics and gynaecology* (ref. 162), 97–98; *idem*, "The haemorrhages", *ibid.*, 104–14; and Medvei, *History of clinical endocrinology* (ref. 7), 262–4.
182. He added, however: "I do not use the word 'invade' in any derogatory sense, for we warmly welcome their aid, which has so long been withheld, but rather because the whole scientific basis of our subject — embryological, morphological, psychological, and physiological, including the biochemical and hormonal, and pathological — has been established by practising obstetricians and gynaecologists here and in other countries". See W. Blair-Bell, "Lloyd Roberts Lecture on the present and the future of the science and art of obstetrics and gynaecology", *British medical journal*, 9 January 1932, 45–50, p. 45.
183. *Ibid.*, 45.
184. *Ibid.*, 49.
185. *Ibid.*, 49.
186. Moscucci, *The science of woman* (ref. 26), 206.
187. Roy Porter and Lesley Hall, *The facts of life: The creation of sexual knowledge in Britain, 1650–1950* (New Haven, Conn., 1995), 173.
188. Blair-Bell, *Sex complex* (ref. 168), 106, 109.
189. W. Blair-Bell, "Disorders of function", in Thomas Watts Eden and Cuthbert Lockyer (eds), *The new system of gynaecology* (London, 1917), i, 287–415, pp. 373, 300.
190. *Ibid.*, 401.
191. Blair-Bell, *Sex complex* (ref. 168), 108.
192. Blair-Bell, "Lloyd Roberts Lecture" (ref. 182), 45.
193. Chris Lawrence, *Medicine in the making of modern Britain, 1700–1920* (London, 1994), 60–61.
194. Steinach's work awaits comprehensive exploration — historians of endocrinology have virtually ignored him, probably because of his involvement in projects considered to be dubious according to the disciplinary dogmas of current endocrinology. (We recall that Herbert Evans's comment

on the obstetric deformation of endocrinology at birth implicated not merely Brown-Séquard but also Steinach.) See, however, Harry Benjamin, "Eugen Steinach, 1861–1944: A life of research", *Scientific monthly*, lxi (1945), 427–42; Marc Klein, "L'oeuvre de Steinach dans l'histoire de la biologie de la reproduction", in Erna Lesky (ed.), *Wien und die Weltmedizin* (Vienna, 1974), 204–13; Chandak Sengoopta, "Glandular politics: Experimental biology, clinical medicine, and homosexual emancipation in fin-de-siècle Central Europe", *Isis*, lxxxix (1998), 445–73; Heiko Stoff, "Die hormonelle und die utopische Geschlechterordnung: Verjüngungsoperationen und der neue Mensch in den zwanziger Jahren", in U. Ferdinand and A. Pretzel (eds), *Verqueere Wissenschaft? Zum Verhältnis von Sexualwissenschaft und Sexualreformbewegung in Geschichte und Gegenwart* (Munich, 1998), 245–60; and *idem*, "Vermännlichung und Verweiblichung: Wissenschaftliche und utopische Experimente im frühen 20. Jahrhundert", in Ursula Pasero and Friederike Braun (eds), *Wahrnehmung und Herstellung von Geschlecht: Perceiving and performing gender* (Opladen, 1999), 47–62. I am currently working toward a comprehensive study of Steinach's work and its contexts, entitled (thanks to Roy Porter!) "A new body for a new age: Eugen Steinach, sexual biology and the regeneration of humanity". Meanwhile, Steinach's self-serving but factually detailed autobiography, *Sex and life: Forty years of biological and medical experiments* (New York, 1940) remains indispensable for an overview of his long, complex and controversial career and should be supplemented with the perceptive discussions in Long Hall, "Biology, sex hormones, and sexism" (ref. 7) and Anne Fausto-Sterling, *Sexing the body: Gender politics and the construction of sexuality* (New York, 1999).

195. In order to study sexual development, it was necessary, of course, to select sexually immature animals; glandular manipulations might lead to visible changes in the sexually mature adult, too — and Steinach's later work would be founded in that fact — but only in prepubertal animals could the ensuing changes be attributed clearly and unequivocally to the experimental procedure.

196. See, for instance, these two foundational texts: Artur Biedl, *Innere Sekretion: Ihre physiologischen Grundlagen und ihre Bedeutung für die Pathologie*, 2nd edn (Berlin, 1913), ii, 199–343; and Marshall, *The physiology of reproduction*, 2nd edn (ref. 170), 320–92. Fausto-Sterling, *Sexing the body* (ref. 194) is alone among recent works in its recognition of Steinach's historical role in the establishment of endocrine theories of sexuality and the contexts and ambiguities of his research.

197. Steinach was nominated for the prize in 1921 by J. H. Zaaijer (Leiden), in 1922 by Y. Sakaki (Fukuoka), in 1927 by L. Haberlandt (Innsbruck), in 1930 by H. H. Meyer, S. Klein and E. Pick (Vienna), in 1934 by A. Durig (Vienna), and in 1938 by J. Bock *et al*. (Copenhagen). My thanks to the Nobel Committee for supplying me with this information.

198. E. Steinach, "Untersuchungen zur vergleichenden Physiologie der männlichen Geschlechtsorgane insbesondere der accessorischen Geschlechtsdrüsen", *Archiv für die gesammte Physiologie*, lvi (1894), 304–38, pp. 337–8; E. Steinach, "Geschlechtstrieb und echt sekundäre Geschlechtsmerkmale als Folge der innersekretorischen Funktion der Keimdrüse", *Zentralblatt für Physiologie*, xxiv (1910), 551–66.

199. Although this very hypothesis had previously been argued by the anatomists Paul Ancel and Pol Bouin from Strasbourg, it was Steinach's espousal of it and his experimental demonstrations that created a controversy, particularly in Central Europe. On the history of this controversy, see Marc Klein, "Sur les interférences des sciences fondamentales et de la clinique dans l'essor de l'endocrinologie sexuelle", *Clio medica*, viii (1973), 31–52, pp. 40–41.

200. This hypothesis was argued most vocally by Josef Halban. See J. Halban, "Die Entstehung der Geschlechtscharaktere: Eine Studie über den formativen Einfluss der Keimdrüse", *AfG*, lxx (1903), 205–308, pp. 260–1. On Halban, see H. H. Simmer, "Josef Halban (1870–1937):

Pionier der Endokrinologie der Fortpflanzung" (ref. 130); and Norbert Pecher, "Halbans Lehre von der protektiven Wirkung der Sexualhormone" (ref. 130).

201. E. Steinach, "Willkürliche Umwandlung von Säugetier-Männchen in Tiere mit ausgeprägt weiblichen Geschlechtscharakteren und weiblicher Psyche", *Pflügers Archiv für die gesammte Physiologie*, cxxxiv (1912), 71–108, pp. 76–77.
202. E. Steinach, "Pubertätsdrüsen und Zwitterbildung", *Archiv für Entwickelungsmechanik*, xlii (1917), 307–32.
203. The relevant section is as follows: "[Meine] fem. masc. hermaph.-Versuche sind keine Geschlechts*bestimmungen*, sondern nur Geschlechts*beeinflussungen* im Sinne der zünftigen Biologen. (Weil das Geschlecht durch die Chromosomen *voraus*bestimmt sei: in praxi nutzt diese 'Bestimmung' aber nichts, da wir das Geschlecht oder wenigstens die Geschlechtsmerkmale *umstimmen* können.)" See Steinach's letter to Harry Benjamin dated 28 February 1923, in Eugen Steinach-Harry Benjamin Correspondence, The New York of Academy of Medicine Historical Collections. (All letters in this collection are filed by date and henceforth, will be cited either as "ES to HB" or "HB to ES", followed by the date.) My thanks to the Academy for making this collection available to me. On the collection's history, see Ernest Harms, "Forty-four years of correspondence between Eugen Steinach and Harry Benjamin", *Bulletin of the New York Academy of Medicine*, xlv (1969), 761–6, and on the career of Benjamin, see the contributions to "Memorial for Harry Benjamin", *Archives of sexual behavior*, xvii, no. 1 (1988), 3–31.
204. On the contexts, connotations, implications and consequences of Steinach's concept of sex-gland antagonism, Long Hall, "Biology, sex hormones and sexism" (ref. 7); and Fausto-Sterling, *Sexing the body* (ref. 194), 159–69.
205. E. Steinach, "Künstliche und natürliche Zwitterdrüsen und ihre analogen Wirkungen: Drei Mitteilungen", *Archiv für Entwickelungsmechanik der Organismen*, xlvi (1920), 12–37, p. 25; *idem*, "Pubertätsdrüsen und Zwitterbildung" (ref. 202), 328–30.
206. E. Steinach and R. Lichtenstern, "Umstimmung der Homosexualität durch Austausch der Pubertätsdrüsen", *Münchener medizinische Wochenschrift*, lxv (1918), 145–8; E. Steinach, "Histologische Beschaffenheit der Keimdrüse bei homosexuellen Männern", *Archiv für Entwickelungsmechanik*, xlvi (1920), 29–37. For a contextual analysis, see Sengoopta, "Glandular politics" (ref. 194).
207. Steinach, *Sex and life* (ref. 194), 22–23.
208. Such endocrine analogies of senility were not unique to Steinach; nor were they confined to the sex glands alone. The signs of hypothyroidism — dry skin, loss of hair, diminished energy — were often considered to be analogous to senility, and there was a proposal to prevent the ravages of old age by administering thyroid extracts prophylactically to women from the age of 35 and to men from the age of 40. For a brief review of this topic, see Humphry Rolleston, *Medical aspects of old age, being a revised version of the Linacre Lecture, 1922* (London, 1932), 79–83.
209. The breed of rats used in the experiment rarely lived beyond 30 months and began to show signs of senility between 18 and 23 months. See E. Steinach, *Verjüngung durch experimentelle Neubelebung der alternden Pubertätsdrüse* (Berlin, 1920), 15.
210. Eugen Steinach, "Untersuchungen über die Jugend und über das Alter" (1912), appendix to *idem*, *Verjüngung* (ref. 209), 61–63.
211. Steinach, *Verjüngung* (ref. 209), 53–60.
212. *Ibid.*, 54–55.
213. For reviews and excerpts of case reports, see Norman Haire, *Rejuvenation: The work of Steinach, Voronoff, and others* (New York, 1925); Peter Schmidt, *The theory and practice of the*

Steinach operation with a report on one hundred cases (London, 1924); and *idem*, *The conquest of old age: Methods to effect rejuvenation and to increase functional activity* (New York, 1931).

214. "Gland treatment spreads in America", *New York Times*, 8 April 1923, sec. 9, p. 2, cols 6–7.

215. Steinach, *Sex and life* (ref. 194), 170–1; Chandak Sengoopta, "'Dr Steinach coming to make old young!': Sex, rejuvenation and the future of humanity", forthcoming.

216. Tissue culture experiments, associated most famously with Alexis Carrel, also suggested that tissues could survive for much longer when cultivated in an artificial environment where their wastes could be washed away. See G. Stanley Hall, *Senescence: The last half of life* (New York, 1922), 285–95.

217. Schmidt, *Conquest of old age* (ref. 213), 27–28.

218. Paul Kammerer believed that the revitalized gonad exerted a stimulatory effect on the other ductless glands: it was the entire, revivified endocrine system that produced the signs of rejuvenation. See Paul Kammerer, *Rejuvenation and the prolongation of human efficiency: Experiences with the Steinach-operation on man and animals* (London, 1924), 219–26. A Prague physiologist reported that the Steinach operation inhibited the progressive 'thickening' of cellular fluids that occurred with ageing (Vladimir Ruzicka, "Die Protoplasmahysteresis und das Verjüngungsproblem", *Deutsche medizinische Wochenschrift*, xlviii (1922), 931–2).

219. Steinach, *Sex and life* (ref. 194), 24–25.

220. Holzknecht was a member of the "world's first independent roentgen department" and, along with his colleagues Robert Kienböck and Leopold Freund, "founded radiology as an independent discipline". See Lesky, *The Vienna Medical School of the 19th century* (ref. 113), 303–4.

221. E. Steinach and G. Holzknecht, "Erhöhte Wirkungen der inneren Sekretion bei Hypertrophie der Pubertätsdrüsen", *Archiv für Entwicklungsmechanik der Organismen*, xxxxii (1917), 490–507.

222. *Ibid.*, 500, where the exact dose of radiation and other technical details can also be found.

223. *Ibid.*, 501. Steinach felt far more confident in identifying the hormone-secreting cells of the ovary than other scientists of the time. On the complexities of ovarian histology, as perceived by Steinach's contemporaries, see Lipschütz, *The internal secretions of the sex glands* (ref. 5), 211–83.

224. See HB to ES, 12 February 1922; on Benjamin's interest in female rejuvenation, see also HB to ES, 27 February 1922. The strong financial incentives attracted Steinach, without completely overpowering him: in 1924, Benjamin sent him a woman for the rejuvenative treatment. Noticing that she had severe anaemia, Steinach sent her home with a prescription for iron and asked her to come back after a year for consideration of further treatment. See ES to HB, 17 May 1924.

225. See, for instance, Manfred Fraenkel, "Die Wirkung der Röntgenstrahlen im Hinblick auf Vererbung und Verjüngung", *Archiv für Frauenkunde und Eugenetik*, vii (1921), 254–63; *idem*, "Zur Theorie der zellfunktionerhöhenden Röntgenstrahlen", *Deutsche medizinische Wochenschrift*, xlviii (1922), 1136–7; F Grödel, "Die Röntgenbehandlung klimakterischer Erscheinungen", *Münchener medizinische Wochenschrift*, lxix (1922), 423–5; and Hans Thaler, "Über Röntgenbehandlung der Amenorrhöe und anderer auf Unterfunktion der Ovarien beruhender Störungen", *CfG*, xlvi (1922), 2034–43. For Holzknecht's rejection, see G Holzknecht, "Gibt es eine Reizwirkung der Röntgenstrahlen?", *Münchener medizinische Wochenschrift*, lxx (1923), 761–2.

226. See H. Benjamin, "The influence of Röntgen rays on the endocrine glands with a contribution to the problem of rejuvenation in women", *Medical journal and record*, cxx (1924), 585–9.

227. *Ibid.*, 586.

228. He also used the radiation technique in men who would not consent to the Steinach operation or could not, for some medical reason, be operated upon. The results were unimpressive. See HB to ES, 20 February 1924.
229. Benjamin, "The influence of Röntgen rays" (ref. 226), 586.
230. See H. Benjamin, "The Steinach method as applied to women", *New York medical journal and medical record*, cxviii (1923), 750–3, p. 751. Benjamin wrote in identical terms to Steinach, adding the crucial thought that psychological factors may have aided in her improvement: "Obgleich Frau A eine sehr nuechterne und durchaus nicht histerische [*sic*] Dame ist, kann ich eine psychiatrische Beeinflussung nicht ausschliessen" (HB to ES, 31 May 1922).
231. Benjamin, "The Steinach method as applied to women" (ref. 230), 752. With another patient, a professional dancer in her late forties, the treatment worked so well that "her husband said she looked as she did twenty years ago. He was so impressed by the change in her appearance that he himself, a man of sixty-one, decided to have the Steinach operation performed" (*ibid.*, 752–3).
232. G. Atherton, *Adventures of a novelist* (London, 1932), 538. A virtually identical sentence appears in Atherton's novel *Black oxen* (New York, 1923), 135. On Atherton, see Carolyn Forrey, "Gertrude Atherton and the New Woman", *California Historical Society quarterly*, lv (1976), 194–209; and Margaret Morganroth Gullette, "Creativity, aging, gender: A study of their intersections, 1910–1935", in Anne M. Wyatt-Brown and Janice Rosen (eds), *Aging and gender in literature: Studies in creativity* (Charlottesville, Virginia, 1993), 19–48, pp. 21–22.
233. Atherton, *Adventures of a novelist* (ref. 232), 539.
234. *Ibid.*, 540.
235. Apart from the frank admission in her autobiography, see also "Rejuvenation: 78-year-old novelist feels 30 years less", *Newsweek*, 14 December 1935, 40; and Atherton, *Black oxen* (ref. 232).
236. Atherton, *Adventures of a novelist* (ref. 232), 542.
237. "Mrs. Atherton causes amusement in Berlin: Newspapers ridicule her suggestion for rejuvenation of all Germany's supermen", *New York Times*, 6 April 1924, Sec. II, p. 7.
238. See ES to HB, 27 October 1923, offering free diathermy treatment to Atherton but warning Benjamin not to reveal the name or the nature of the new treatment to her; see ES to HB, 20 August 1934, for Steinach's appreciative comments on Atherton's referrals. When Steinach began to use his hormonal preparation Progynon for rejuvenating women, Benjamin, on his advice, treated her with high doses of it (see HB to ES, 14 September 1934 and ES to HB, 25 August 1937). There are records of other women patients (at least one of whom was quite well-known) in the Benjamin-Steinach correspondence but for ethical reasons I have avoided discussing their cases here. Atherton's own open avowals of her treatment(s), of course, free me from such considerations.
239. Benjamin, "The influence of Röntgen rays" (ref. 226), 587; HB to ES, 26 November 1923. Steinach was extraordinarily secretive about his diathermy technique and never published on it, in spite of Benjamin's advice to do so in order to secure priority (HB to ES, 26 November 1923). The letters do not contain much detail either (with the partial exception of ES to HB, 23 October 1923) but are full of warnings from Steinach about maintaining utter secrecy: see, for instance, ES to HB, 11 September 1923, and for Benjamin's promise to maintain strict silence, HB to ES, 5 October 1923. After a trip to Vienna in 1923, however, Benjamin was obviously well-versed in the technique: one can assume, therefore, that he had been trained in person by Steinach himself. See HB to ES, 17 August 1923. The shroud of secrecy was never lifted by Steinach, although his Berlin disciple Peter Schmidt published an enthusiastic account of the diathermy technique in his 1928 book on rejuvenation, which was translated in 1931 as *The conquest of old age* (ref. 213) (92–98). Diathermy was often combined with the administration of very

small doses of yohimbine and radium, which, Steinach believed, heightened the response of the glandular tissue. See ES to HB, 11 September 1923.

240. See HB to ES, 24 April 1924; HB to ES, 12 November 1924. Steinach was none too happy about Benjamin's adventurousness in applying the x-ray treatment. In March 1922, he wrote sharply, warning him that he was not experienced with x-rays clinically or theoretically — only two or three people were really well-versed in the demanding art of choosing appropriate cases for x-ray rejuvenation and administering the treatment. It was only too easy to bring about total castration and hormonal deficiencies if the dosage was even slightly beyond that required. He himself had not yet had too many impressive cases (ES to HB, 29 March 1922). Benjamin soothingly replied that he was administering the treatment under the guidance of first-class radiologists: see HB to ES, 21 April 1922.
241. Schmidt, *Conquest of old age* (ref. 213), 97–98.
242. See George F. Corners [pseudonym of George Sylvester Viereck], *Rejuvenation: How Steinach makes people young* (New York, 1923), 82; and William Wolf, *Endocrinology in modern practice*, 2nd edn (Philadelphia, 1939), 230.
243. Schmidt, *Conquest of old age* (ref. 213), 31–32, 291.
244. "In the future, aging enchantresses, desirous of retaining their charms, will combine the [Steinach irradiation] treatment with plastic surgery", predicted the Steinach enthusiast George Viereck. "Each complements the other. The plastic surgeon accomplishes his end more quickly than the slow process of nature set in motion by the revived Puberty Gland. On the other hand, the natural reserves invoked by the x ray, make the effect of his operation more lasting". See Corners, *Rejuvenation* (ref. 242), 83. The link between rejuvenation and plastic surgery seems to have been close in the 1920s: a pioneer cosmetic surgeon of London described his fat-injection technique as "facial rejuvenation", comparing it with glandular surgery. See Tim Armstrong, *Modernism, technology and the body: A cultural history* (Cambridge, 1998), 100; and Elizabeth Margetson, *Living canvas: A romance of aesthetic surgery* (London, 1936). On the history of plastic surgery in general, see Sander L. Gilman, *Making the body beautiful: A cultural history of aesthetic surgery* (Princeton, NJ, 1999).
245. Harry Benjamin, "The story of rejuvenation", *American Mercury*, December 1935, [2]. Cited from independently paginated reprint in the Benjamin–Steinach correspondence (Box: "Eugen Steinach, biography, photographs, articles, letters 1920–1927"; Folder: "Biographies of Steinach").
246. Atherton, *Black oxen* (ref. 232), 176.
247. See Stoff, "Die hormonelle und die utopische Geschlechterordnung" (ref. 194), 246–7.
248. Barbara Spackman, *Decadent genealogies: The rhetoric of sickness from Baudelaire to D'Annunzio* (Ithaca, NY, 1989), 30.
249. On these themes, see Sengoopta, *Otto Weininger* (ref. 104); *idem*, "Glandular politics"(ref. 194).
250. On the German discourse, see Iwan Bloch, *Das Sexualleben unserer Zeit im Lichte der modernen Kultur* (Berlin, 1908), 41–56; Annemarie Wettley and Werner Leibbrand, *Von der 'Psychopathia sexualis' zur Sexualwissenschaft* (Stuttgart, 1959), 70–76; and Frank J. Sulloway, *Freud, biologist of the mind: Beyond the psychoanalytic legend* (New York, 1979), 158–60, 292–6. On the British discourse, see Ornella Moscucci, "Hermaphroditism and sex difference: The construction of gender in Victorian England", in Marina Benjamin (ed.), *Science and sensibility: Gender and scientific enquiry* (Oxford, 1991), 174–99. For a suggestive analysis of the theme of universal androgyny, see Lawrence Birken, *Consuming desire: Sexual science and the emergence of a culture of abundance, 1871–1914* (Ithaca, NY, 1988); and for a comprehensive exploration of one conceptually and clinically crucial theme, see Alice D. Dreger, *Hermaphrodites and the medical invention of sex* (Cambridge, Mass., 1998). I am

presently working on a comprehensive history of the concept and its contexts with the working title "Between the sexes".

251. I say "almost" because there was some ambivalence about the desirability of complete sexual differentiation within certain clusters of discourse, notably the homosexual emancipationist discourse associated with the German homosexual doctor Magnus Hirschfeld. Briefly, the work of Hirschfeld and his associates was characterized by the claim that male homosexuals were biologically feminized and, therefore, they were neither ill nor vicious in desiring intercourse with other men. Homosexuals, Hirschfeld insisted, simply represented a somewhat extreme instance of universal 'bisexuality': pure masculinity or pure femininity were theoretical ideals — all existing individuals were 'bisexual', mixtures of male and female qualities in degrees that varied from individual to individual. Nevertheless, even Hirschfeld's work was pervaded by the acceptance of the categories of male and female in their traditional, normative terms. For a detailed analysis of Hirschfeld's ambivalence and its wider contexts, see Sengoopta, "Glandular politics" (ref. 194).
252. K. Kraus, "Ich las es so", *Die Fackel*, cccc–ccccciii (1914), 68–69; and Magnus Hirschfeld, "Die Untersuchungen und Forschungen von Professor E. Steinach über künstliche Vermännlichung, Verweiblichung und Hermaphrodisierung", *Vierteljahresberichte des Wissenschaftlich-humanitären Comitées/Jahrbuch für sexuelle Zwischenstufen*, xvii (1917), 3–21. On Mrs Steinach's opinion, see Corners, *Rejuvenation* (ref. 242), 27–28 and George Sylvester Viereck, *Glimpses of the great* (New York, 1930), 256.
253. See Stoff, "Die hormonelle und die utopische Geschlechterordnung" (ref. 194), 331.
254. See Paul Kammerer, *Rejuvenation and the prolongation of human efficiency* (ref. 218), 189; and Corners, *Rejuvenation* (ref. 242), 94–95.
255. The Schering website seems to be the only prominent locus where the name of Steinach is still remembered with appreciation. See http://www.schering.de/unternehmen/Historie/ChronikderScheringForschung2.htm (my thanks to Angela Dahrmann and Christine Berghausen of the Schering Museum for their assistance).
256. Corner, "Early history" (ref. 6), pp. ix–x. See also H. H. Simmer, "On the history of hormonal contraception, II: Ottfried Otto Fellner (1873–19??) and estrogens as antifertility hormones", *Contraception*, iii, no. 1 (1971), 1–20. Iscovesco as well as Fellner are remarkably elusive figures; neither Corner nor Simmer succeeded in discovering much about their lives or their work, beyond that reported in their published papers.
257. Corner, "Early history" (ref. 6), p. xi.
258. *Ibid*.
259. J. Süß, "Die Ein-Hormon-Hypothese: Eine aufschlußreiche Episode in der Geschichte der Ovarialendokrinologie", *Geburtshilfe und Frauenheilkunde*, xlvii (1987), 134–7.
260. Süß, *ibid.*, 135.
261. HB to ES, 3 November 1939. See also Long Hall, "Biology, sex hormones and sexism" (ref. 7).
262. Viereck, *Glimpses of the great* (ref. 252), 264.
263. See Dorothy Price, "Feedback control of gonadal and hypophyseal hormones: Evolution of the concept", in J. Meites, B. T. Donovan and S. M. McCann (eds), *Pioneers in neuroendocrinology* (New York, 1975), i, 217–38. As Price emphasized, the feedback theory developed from the biologist Carl Moore and her own efforts to disprove Steinach's concept of sex-gland antagonism. For a recent, astute analysis, see Fausto-Sterling, *Sexing the body* (ref. 194), 164–69.